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The Heresthetics of Constitution-Making: The Presidency in 1787, with Comments on Determinism and Rational Choice

Presidential Address, American Political Science Association, 1983

WILLIAM H. RIKER
University of Rochester

One contemporary method of reconciling the conflict in methodology between determinism and indeterminism is the notion of rational choice, which allows for both regularities in behavior and artistic creation. A detailed explanation of artistry within the rational choice context has not yet been developed, so this essay offers such an explanation in terms of the notion of heresthetics or the dynamic manipulation of the conditions of choice. The running example used throughout is the decision on the Constitutional Convention of 1787 on the method of selecting the president.

The Philosophical Issue

Looking backward at the climax of some earlier social event, for example, at the decision of a committee, it often appears to have been inevitable, as if there were just one admissible outcome and as if the committee were driven to that outcome by its composition, its internal customs, and its external constraints. A history written from this deterministic point of view admits no human choice and no element of chance and describes each step in the causal chain as forcing, with certainty, each succeeding step toward the decision.

It is true that, once an event has happened, an alternative event with a different outcome cannot occur, at least not in this world. But before the event has begun or is over, its outcome does not seem so inevitable. For example, if a committee has eleven members and operates by majority rule, then there are over a thousand winning coalitions. Each one, with its different personnel, might produce a different outcome. Furthermore, if the subject of decision allows for several different standards of judgment, then it is almost certain that no outcome is preferred by a majority to all others (McKelvey, 1976, 1979). In other words, for every outcome, *p*, that some majority might adopt, there is another outcome, *q*, supported by another majority, that can beat *p*. In this sense there may be no predictable equilibrium outcome(s) toward which the committee moves by reason of internal forces and members' tastes. Rather, with many procedurally equivalent possibilities, among which prediction seems difficult, the world seems quite undetermined. That a particular outcome occurs is a function of the decisive coalition that happens to exist at the time of decision, of the outer boundaries in a policy space of members' most preferred alternatives, of the sequence in which alternatives are considered, and of many other constraints, including accidents, on the decision-making process. And until a decision is actually made, there is some probability for each of a set of possible outcomes.

Neither of these viewpoints, determinism or indeterminism, is philosophically attractive. Although determinism easily admits scientific generalization, it denies choice and artistry, which, however, many people believe they experience. They may merely confuse an indecipherable complexity with freedom of the will. Yet their sense of invention is still very lively. Similarly, determinism denies natural accident, which may be no more than an incapacity to measure, but which may also reflect genuine randomness in the world.

On the other hand, although indeterminism provides for choice and chance, it denies the possibility of generalizing about social outcomes. Yet the idea at the core of social science is that outcomes can be subsumed under general laws. And they are. For example, the well-attested law of demand (namely, that, with appropriate conditions, demand curves do not slope upward), can be used for many kinds of accurate predictions. So although social science is, of course, only modestly successful, it does have some triumphs, and a philosophical viewpoint that denies them is clearly untenable.

A third viewpoint, which circumvents the defects of both determinism and indeterminism, is the method of rational choice. This is in fact the traditional paradigm in political science, although in the absence, until recently, of a well-developed political theory (Riker, 1982a,b, 1983), most of those who subscribed to the paradigm were not conscious of the nature of their allegiance. The fundamental assumption of this paradigm is that people maximize expected utility, namely that, given their goals, they choose those alternatives likely to result in the largest net achievement of goals. The scientific merit of the assumption is that it allows both for regularities and for freedom of choice. Since presumably all persons with the same goals in the same circumstances rationally choose the same alternative, regularities can be observed. Inasmuch as social institutions impose similar circumstances on persons with similar goals, the role of randomness is minimized, but the role for choice is fully preserved. Thus, generalization and social science are reconciled with choice and chance.

Merely to assert the reconciliation does not, however, explain how artistic creation can be subsumed under scientific generalization. To begin the reconciliation I point out that, for any reasonably complicated situation, the range of alternatives and outcomes is tremendous. Furthermore, often no alternative or outcome dominates (i.e., is socially chosen over all others), so that there is no obvious rational choice. The problem, then, is to explain how, in the face of apparent disequilibrium, regularities can be discerned and, simultaneously, creativity recognized (Fiorina & Shepsle, 1982; Riker, 1980). The first step, which is what social science has traditionally been concentrated on, is the identification of constraints imposed on possible outcomes by institutions, culture, ideology, and prior events. The next step, which rational choice models provide for, is the identification of partial equilibria from utility maximization within the constraints. The final step, to which this essay is addressed, is the explication of participants' acts of creative adjustment to improve their opportunities.[1]

Conventional political science has provided considerable understanding of institutions, the first step. (For the history of an example of one such development, see Riker, 1982b.) Recent elaboration of rational choice theory, from Duncan Black to the present, has provided understanding of the second step, utility maximization in a political context. Unfortunately, not very much effort has been devoted to the third step, the study of creative adjustment, or what I have elsewhere described as heresthetics, the art of political strategy (Riker, 1983). Because this third step has hitherto been neglected, I shall devote this essay to the detailed study of one creative act with the intention, not simply of explicating it, but also of explaining how it can be placed within models of partial equilibria.

Background of the Event

The act to be studied is the decision, at the Federal Convention of 1787, on the provisions of the Constitution for the choice of the president. Admittedly this subject was not as politically intense or important as several others about which delegates either withdrew or threatened to withdraw. Nevertheless, the decision on the choice of the executive is appropriate for analyzing creativity because it was protracted—it recurred all summer; confusing—the decision was changed several times; and multifaceted—it involved consolidation and equality of states as well as the separation of powers. Near the end of the Convention, with the subject still unsettled, James Wilson (Pa.) remarked exasperatedly, "This subject has greatly divided the House, and it will also divide people out of doors. It is in truth the most difficult of all on which we have had to decide" (Farrand, 1966, II, 501).[2] An exaggeration, of course, but an understandable one. Part of the difficulty was that many methods were feasible, and some were shown to be in a cycle. Another part was that alternative methods were invented and revised, nearly to the very end of the Convention. The successive invention, revision, and elimination of alternatives is a distinctive feature of the dynamics of social choice. I call it heresthetics (Riker, 1983), or the method and art of influencing social decisions. Through heresthetical manipulation, individual artistry enters decision making. Hence, the analysis of the development of an agenda which, like this one, was heresthetically manipulated, is an excellent vehicle for the explication of political creativity.

As a preface to the analysis of dynamics, it is useful to place before us the full set of possible outcomes, as they would appear in a static analysis:

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[1] In economics one can easily identify these three steps with, first, the analysis of institutions like markets and auctions, with, second, the analysis of trading equilibria (i.e., Walrasian general equilibrium analysis or Edgeworthian study of the core of exchanges relations), and with, third, the Austrian emphasis on the dynamic role of entrepreneurs in the expansion of economic systems. I owe this analogy to William Mitchell.

[2] Hereafter, Farrand (1911/1966) is cited simply with volume and page number, and, if relevant, the date. All quotations of speeches are Madison's *Notes* and are Madison's precis of what was actually said. See Jillson (1979) for another version of the event studied here.

1. Election by the national legislature or some subset of it, analogous to the method of electing governors in most states;
2. Election by the people, or by electors chosen by the people, analogous to methods of electing governors in New York and in the New England states;
3. Election by some institutions of state government, analogous to the election of members of Congress under the Articles of Confederation;
4. Selection by various combinations of elements from the foregoing categories.

The method actually chosen was from the fourth category (i.e., by electors chosen in a manner prescribed by state legislatures, with election by the House of Representatives in the absence of a majority among the electors), as is the method used today (i.e., by electors popularly elected in states, with the same method of breaking ties). Although our present method was not thought of at the Convention, something very similar was rejected.

Another possible method, certainly technologically feasible in 1787, was hereditary succession, but for ideological reasons the framers did not consider it.

Altogether, we can arrange the alternatives in sets, thus:

1. Alternatives the framers discussed or voted on;
2. Feasible alternatives, which include all four previous categories;
3. Possible alternatives, which include all feasible alternatives as well as infeasible ones like hereditary succession.

Considered alternatives is a proper subset of *feasible alternatives,* which is a proper subset of *possible alternatives.*

I turn now to a discussion of how considered alternatives were brought up and a decision reached among them.

Election by National Legislature

The first alternative, placed before the Convention in the Virginia plan, was election by the national legislature. This alternative had substantial support, was in fact adopted three times, and was incorporated into the penultimate draft. But it also had substantial opposition, which repeatedly invented substitutes. In the end an invention won.

The reason for the popularity of the original alternative was, I believe, that it harmonized with the nationalism of the Virginia plan, which served as the basis for discussion and, ultimately, as the skeleton of the Constitution itself. In essence, the Virginia plan provided for a strong national government, politically separated from the states. To preserve the separation, the federal government was to administer its own laws, to supervise state legislation, and to be mostly independent of the states for renewal. It was to have a tripartite form:

1) a bicameral legislature, with a popularly elected branch and a smaller branch, elected by the first, though nominated by state legislatures, and with the right to negative state laws;
2) an executive elected by the national legislature and constituting (with some of the judiciary) a council of revision over legislative action;
3) a judiciary chosen by the national legislature.

The finished Constitution is very much like the Virginia plan, changed only in these main particulars: *representation in and election of the Senate,* where finally the Senate was to represent states equally and to be elected by state legislatures; *election of the president,* who finally was to be elected by electors—chosen in a manner to be prescribed by state legislatures—and, in the event of no majority, by the state delegations in the House of Representatives; *council of revision,* which, though eliminated, survived as the presidential veto; and, *negative on state legislation by the national legislature,* which though eliminated, survived in the supremacy clause. Aside from the council of revision, these modifications either grant state governments a role in selecting federal officials or liberate state governments from federal supervision. By so doing, they obscure the intense nationalism of the Virginia plan. This plan had provided for a separate national government which would supervise the states, but the modifications in Convention blurred it with concessions to provincialism.

The reason the Virginians made this extreme proposal was, I believe, their conviction that state legislatures were incompetent and unjust. Earlier, in the spring of 1787, Madison, their intellectual leader, has written a widely circulated manuscript, *Vices of the Constitutions of the United States,* in which state legislatures were blamed both for the weakness of the national government and for ten of the eleven "vices" (e.g., "injustices of the laws of the states") (Madison, IX, 345-358). In the Convention he ultimately deserted election of the executive by the national legislature because it might be like state legislatures which "had betrayed a strong propensity to a variety of pernicious measures" (II, 110, 25 July). So deeply did he distrust state legislatures that, throughout the Convention, he pushed for a national negative on state laws (see, especially, I, 164, 169; II, 27, 589) and, afterward, writing to Jefferson, he described the Constitution pessimistically because it did not include the negative (Hobson, 1979; Madison, X, 211-212, October 24). The other Virginian delegates similarly despised state legislatures and indeed state governments generally. In Madison's record of the vote (8 June) on a motion to extend the national negative on state laws from "unconstitutional" laws to "improper" laws (I, 168, roll call 34), he noted that Blair, McClurg, and he voted for this extreme form of the negative, whereas Mason and Randolph voted nay. (Washington was not consulted, which probably means he was regarded by his colleagues as favoring Madison's position. Had Washington not, his vote would have produced a tie and would therefore have been obtained.) But Mason and Randolph also distrusted state governments, even though they were not willing to push the negative as far as the others. During the Convention, Mason frequently disparaged state legislatures.[3] And Randolph based his opposition to a departure from the Virginia plan on his dislike of state power: "A Natl. Executive thus chosen (i.e., by state executives) will not be likely to defend with becoming vigilance & firmness the national rights agst. State encroachments" (I, 176). Furthermore, he maintained his opposition to all methods other than by the national legislature right to the end of the summer (II, 502).

Most of the other delegates shared the Virginians' distaste for state governments. Of the 55 delegates, only four could be regarded as genuine Anti-Federalists, that is, wholehearted supporters of provincial political establishments. These were Yates and Lansing of New York, who, however, went home on 10 July, Mercer of Maryland, who was present only two weeks in August, and Luther Martin of Maryland, the only one of the four to participate long and actively. (Gerry of Massachusetts, Mason, and Randolph ultimately refused to sign, but they had, until near the end, cooperated fully in planning a stronger federation.) Every other delegate who stayed to the end and probably most of those who went home early either had federalist sympathies throughout the Convention or acquired them during the summer. Even the small state diehards, who fought for a role for state legislatures in electing the Senate, cared very little for the state governments themselves. They wanted simply to protect Connecticut and New Jersey against New York, Delaware against Pennsylvania, Maryland against Virginia, etc. As C. Pinckney (S.C.) predicted, cynically and correctly: "Give New Jersey an equal vote, and she will dismiss her scruples, and concur in the National system" (I, 255).

As a consequence of federalist sympathies, most delegates appreciated the Virginians' intention to avoid, as much as possible, allowing state governments a part in selecting federal officials. Although those who favored election by the national legislature seldom rationalized their position, they adhered to their preference through most of the summer, mainly I believe, because of their intense conviction that the provincials should be supervised by the national elite. Accordingly, the less elitist ultimately abandoned the Virginia plan, while the most elitist persisted, right up to the end of the summer, in favoring election of the executive by the national legislature, e.g., Mason (Va.), Randolph (Va.), Williamson (N.C.), Rutledge (S.C.), and C. Pinckney (S.C.).

Opposition to Legislative Election

But this method was ultimately opposed by people like Gouverneur Morris (Pa.) who believed that, "of all possible modes of appointment that by the Legislature is the worst" (II, 103). The opponents were just as federalist as Madison, but they had different expectations about how the Virginia proposal would work out.

To begin with, the opponents attributed the vices of state governments to legislative supremacy, not simply to provinciality. They agreed, of course, with Madison that state legislatures were reckless, myopic, and fiscally irresponsible. But in their view the worst problem was that the legislatures were unrestrained. Despite the lipservice in the state constitutions to the idea of the separation of powers, in most states the governors were elected by the legislature and thus subordinate to it, as were indeed the popularly elected governors in New England and New York. Most of the delegates probably agreed with Madison's remark, "Experience has proved a tendency in our governments to throw all power into the Legislative vortex. The Executives of the States are in general little more than Cyphers; the legislatures omnipotent" (II, 35).

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[3] His bluntest remark, uttered in support of the executive veto, justified it on the ground that the national legislature would "resemble" state legislatures and would therefore frequently "pass unjust and pernicious laws" (II, 78). Mason is often supposed to have been a principled Anti-Federalist because he refused to sign the Constitution. But as Ellsworth (Conn.) alleged, Mason's motive was not to defend state governments, but rather to support a provision for a regional economic interest, namely a provision to prevent eastern monopoly of the carrying of southern agricultural exports (Ford, 1892, pp. 161-162; *The Landholder* V, 10 Dec. 1787). In a letter to Jefferson in 1788 and again in conversation with him in 1792, Mason himself explained his defection on the same grounds (III, 304-305, 26 May 1788; III, 367, 30 Sept. 1792).

It follows from this diagnosis that the appropriate constitutional structure is not merely the separation of the federal executive from the states, as in the Virginia plan, but also and especially the separation of the federal executive from the federal legislature, as James Wilson (Pa.) initially made clear when he "renewed his declarations in favor of an appointment by the people. He wished to derive not only both branches of the Legislature from the people, without the intervention of the State Legislatures but the Executive also; in order to make them as independent as possible of each other, as well as of the States" (I, 69).

Both nationalism and an ideal of separated powers stand behind this remark. Madison himself, once he was converted to Wilson's view, intellectualized it—both in the Convention and in the 51st *Federalist*—in the way it has survived in the constitutional tradition (II, 34):

> If it be essential to the preservation of liberty that the Legisl: Execut: & Judiciary powers be separate, it is essential to a maintenance of the separation, that they should be independent of each other. The Executive could not be independent of the Legislature, if dependent on the pleasure of that branch for a reappointment. . . .

Thus justified, the mode ultimately adopted of electing the President is an essential institution for the ideal of the separation of powers, which is, perhaps, one reason why the nonseparationist Virginia method was rejected.

But although a desire for consistency may have predisposed the delegates to some alternative method, the positive force came from Pennsylvanians, especially Wilson and G. Morris. Pennsylvania was the only state with a unicameral legislature and thus wholly lacking in an institutional expression of the separation of powers. In its domestic politics, the unchecked legislative supremacy was so central an issue that one of the names of the political parties derived from it: the Constitutionalist (populist) and the Republican (anticonstitutional and liberal). Since most Pennsylvania delegates were Republican, the Convention had a core of distrust of legislative supremacy from the very beginning. Wilson initiated the attack on the Virginia method and G. Morris, an adept and persistent heresthetician, ultimately maneuvered the Convention into adopting the electoral college. So it is to them, I think, that the bulk of the credit should go for inventing and fleshing out this institution.

Displacement of the Method of Legislative Election

I turn now to an analysis of the rhetorical and heresthetical strategy by which the Virginia proposal was displaced. After the Convention was organized (25-29 May), the procedure to deal with the Virginia plan was:

Stage 1. Committee of the Whole (29 May to 13 June): to consider the Virginia resolutions and report them, as revised, to the Convention;

Stage 2. Report of the Committee of the Whole (15 June to 26 July): to consider the resolutions of the Report and refer them, as revised, to the Committee of Detail;

Stage 3. Report of the Committee of Detail (6 August to 10 September): to consider the Articles of the Report and refer them, as revised, to the Committee on Style;

Stage 4. Report of the Committee on Style (12-17 September): to revise the Articles of the Report and adopt the Constitution.

The drama of events on Presidential election was that in Stage 1 the nationalistic Virginia proposal was adopted 8-2 (roll call 12, I, 79) but near the end of Stage 3 it was rejected 2-8 (roll call 445, II, 508) with one divided, and a separation of powers proposal was adopted 9-2 (roll call 457, II, 520). In brief, the course of that remarkable reversal was: In Stage 1 the separationists offered only minor opposition. In Stage 2, although nationalists easily defeated a motion for popular election, the separationists responded with a clever rhetorical and heresthetical attack. Then they coalesced with the small-state interest, fresh from its triumph on the equality of states in the Senate, in support of an electoral college chosen by state legislatures. This motion passed, but then the nationalists came back (with support from delegates from distant states) and displaced the college with the original Virginia method. Thus ended Stage 2. But in Stage 3, after initial failures for popular election and for electors, the matter was referred to a Committee of Eleven, composed mostly of separationists, including especially G. Morris. Its report was the substance of our present electoral college, which satisfied the small state interest because election was initially to be in a college chosen by state legislatures and ultimately in the Senate, which satisfied the delegates from distant states because a state's electors were to meet in the state capital, and which satisfied, most of all and fundamentally, the separationists because it avoided election by the national legislature. A diehard nationalist motion to substitute the Virginia proposal for the college failed decisively, and the college was adopted by a large majority. It, of course, survived with minor revisions into Stage 4 and the Constitution as adopted.

The Method of Popular Election

The separationists probably preferred some kind of popular election, either direct or by electors. But they could never muster enough support for such proposals on their own, even though they tried four times, so they were inspired both to launch a rhetorical and heresthetical attack and to coalesce with the small state interest. Thus, the first step along the road to the adoption of the electoral college is the repeated failure of both kinds of popular election.

Although support for popular election increased over the summer, as indicated in Table 1 (i.e., from roll call 11 to roll call 359), it could not ultimately win, as indicated in roll call 361. Why not? The short answer is, I think, that the arguments for it simply were not persuasive for the majority who found it distasteful. That it was distasteful even its most convinced proponents recognized. Wilson, for example, introduced his motion (roll call *11*) with the admission that he was "almost unwilling to declare" his preference because he was "apprehensive that it might appear chimerical" (I, 80). As for others, some thought popular election unwieldy (Butler (S.C.), II, 112; King (Mass.), II, 56); others thought the people incapable of judging executive talent (Sherman (Conn.), II, 29; Williamson (N.C.): "There was the same difference between election . . . by the people and by the legislature, as between an appointment by lot, and by choice" II, 32; Mason (Va.): "It would be as unnatural to refer the choice of a Chief Magistrate to the people, as it would, to refer a trial of colours to a blind man" II, 31; and still others feared the people would be misled by demagogues (C. Pinckney (S.C.), II, 30; Mason, II, 119; Gerry (Mass), II, 57, where the typical demagogues are on the left, i.e., the Shaysites who turned out Governor Bowdoin for doing "his duty," and II, 114, where the typical demagogues are on the right, i.e., the Cincinnati). But probably the most telling argument was uttered by Madison, speaking ostensibly in favor of popular election: "the right of suffrage was more diffusive in the Northern than the Southern states, and the latter could have no influence in the election on the score of the Negroes" (II, 57). The effect of this viewpoint is seen in the fact that North Carolina, South Carolina, and Georgia never voted for popular election and in the fact that Ellsworth immediately picked up on Madison's reiteration of his argument, saying that the "objection from different sizes of states was unanswerable" (II, 111).

Table 1. Motions for Popular Election of the President

Date and Stage	Roll Call Number and Page	Movers	Motion	Outcome	Yea	Nay	Divided
2 June Stage 1	11 1, 79	Wilson	Electors, popularly chosen in districts	2-7-1 (N.H., N.J. absent)	Pa., Md.	Mass., Conn., Del., Va., N.C., S.C., Ga.	N.Y.
17 July Stage 2	165 II, 24	G. Morris	Citizens of U.S.	1-9 (N.H., N.Y. absent)	Pa.	Mass., Conn., N.J., Del., Md., Va., N.C., S.C., Ga.	
24 August Stage 3	355 II, 399	Carroll Wilson	People	2-9 (N.Y. absent)	Pa., Del.	N.H., Mass., Conn., N.J., Md., Va., N.C., S.C., Ga.	
24 August Stage 3	359 II, 399	G. Morris Carroll	Electors chosen by people of several states	5-6 (N.Y. absent)	Conn., N.J., Pa., Del., Va.	N.H., Mass., Md., N.C., S.C., Ga.	
24 August Stage 3	361 II, 399	G. Morris	Electors	4-4-2 (Mass., N.Y. absent)	N.J., Pa., Del., Va.	N.H., N.C., S.C., Ga.	Conn., Md.

On the other side, there was very little to recommend popular election. The best argument was practical: that it would guarantee the election of a person of "general notoriety," "continental reputation," and "distinguished character," polite references to Washington who was sitting up front of the room (by Wilson, I, 68, G. Morris, II, 29, and Madison, II, 57, and II, 111). But the delegates were, of course, practically certain that Washington would be offered the office whatever the method, so this was far from a conclusive argument.

Relatively few persons can be identified as favoring popular election. From quotations in Madison's *Notes* we know about: Wilson, G. Morris, Madison, Carroll (Md.), Dickinson (Del.), Franklin (Pa.), and possibly King (Mass.). Beyond that we know that Jenifer (representing Maryland by himself) voted yea on vote 11, but apparently he then changed his mind, because Maryland voted nay on votes 165, 355, and 359. Had Jenifer voted yea on any of these, his vote with Carroll's would have divided Maryland, as it probably did on vote 361. We also know that Hamilton must have voted yea on vote 11—on which New York was divided—because only he and Yates then represented New York, and Yates must surely have voted nay. But the New Yorkers went home before any other votes in Table 1 occurred. Dickinson must have persuaded at least two others from Delaware to vote yea on 24 August, although the record of Delaware is erratic on that day. Morris, Wilson, and Franklin must have been joined consistently by at least one other from Pennsylvania. And for votes 359 and 361 Madison must have persuaded Blair and Washington, because surely Mason and Randolph were too devoted to the Virginia plan to approve popular election. Connecticut also voted yea on 359 and was divided on 361, which means that Sherman and Johnson once agreed and once split on a yea vote. Finally, on 24 August, New Jersey was represented by Brearley, Dayton, and Livingston, at least two of whom must have voted yea on 359 and 361. Altogether, then, at its best showing, no more than about 14 to 20 delegates (out of 42 then in Philadelphia) approved of popular election. These were concentrated in four delegations: Pennsylvania, Delaware, Virginia, and New Jersey. Furthermore, it is likely that delegates from Connecticut and New Jersey, who voted for electors but not for direct popular election, were mainly interested in electors as a way to increase the influence of small states and only incidentally supporters of the extreme separationist doctrine. If so, no more than 11 to 17 favored popular election itself.

As against these, we know, by inference from Madison's *Notes* and from the number present in each delegation, the number required for the delegation to vote, and the vote actually cast, that toward the end of the summer two of the five delegates from Virginia, two of the three from Massachusetts, three of the four from Maryland, and all the delegates from New Hampshire, North Carolina, South Carolina, and Georgia were solidly against popular election in any form. This is a total of at least 19 delegates controlling at least 6 of the 11 delegations. Clearly, the method of popular election could not have been adopted.

The Rhetorical Attack: "Intrigue"

My count, arrived at by immersion in detail, was immediately evident to the separationists. Once aware of their shortage of votes—on 17 July when the Convention took up the election of the executive in Stage 2—they responded with the argument from the doctrine of the separation of powers. G. Morris was first: "If the Executive be chosen by the Natl. Legislature, he will not be independent on it; and if not independent, usurpation & tyranny on the part of the Legislature will be the consequence" (II, 31). Others reiterated his theme: G. Morris (Pa.) himself: II, 52, 103-104, 112, 500; Madison (Va.), by then a convert to the separationist cause: II, 34-35, 56-57, 109; McClurg (Va.): II, 36; King (Mass.): II, 67; Wilson (Pa.): II, 102-103; and Dickinson (Del.): II, 114.

The structure of this argument is the inference of an institutional form from an accepted principle of philosophy. Such abstract arguments are not universally convincing among practical men of affairs. More persuasive, probably, are simple analogies with well-known situations that the auditors agree are undesirable. Exactly such an analogy was available in the widely accepted proposition that legislative election of executives involved "intrigue." By "intrigue" the framers probably meant no more than maneuvering to form cabinets in fragmented parliamentary systems. (The one concrete example was G. Morris's discussion of the intrigue by which, in 1784, Pitt came to office by defeating Fox's India bill (II, 104).) This now-ordinary behavior is, however, what the framers found distasteful because it seemed to place office above principle, and hence, for the separationists, it was an appropriate symbol of the evil in legislative election.

That many framers disapproved of parliamentary "intrigue" cannot be doubted. Of course the separationists referred to it often, gradually substituting Morris's striking phrase "cabal and corruption": G. Morris (Pa.): II, 31, 103-104, 112, 403; "cabal and corruption" II, 500, 501; Madison (Va.): II, 109-111; Wilson (Pa.): II, 30, 32, 103, 501. But others, who were not separationists, also seemed fearful of intrigue and cabal, which indicates the probable effectiveness of this analogy: Mason (Va.), who used the word "intrigue" in the first discussion of election of the executive: I, 68, 1 June; I, 86; II, 500; Gerry (Mass.): I, 80, 175; Randolph (Va.): II, 54-55; Butler (S.C.): II, 112, 501; Williamson (N.C.): II, 113, 501; and Hamilton (N.Y.): II, 524-525. At the end of the Convention, G. Morris identified the "principal advantage" of the electoral college as "taking away the opportunity for cabal." Presumably, he thought this was the most important rhetorical theme, not only for himself but also for his auditors.

The Heresthetical Attack: McClurg's Motion

Although it doubtless was rhetorically desirable to reiterate the threat of intrigue, it probably appeared even better to link "intrigue" logically with the Virginia proposal. The separationists accomplished this by what seems a consciously heresthetic maneuver. (In this connection the distinction between rhetoric and heresthetic is that rhetoric involves converting others by persuasive argument, whereas heresthetic involves structuring the situation so that others accept it willingly.) Here the maneuver was to induce acceptance of the proposition that, if elected by the legislature, then, in order to avoid intrigue, the executive should have a long term and be ineligible for a second one. Mason himself had assumed the linkage when, in Stage 1, he proposed a seven-year term to minimize "a temptation . . . to intrigue" (I, 68). Then in Stage 2, just after the rejection of popular election, G. Morris (Pa.), Houston (Ga.), and McClurg (Va.) conducted a remarkable heresthetic maneuver to reimprint the stigma (II, 32-35).

On 17 July the Convention unanimously reaffirmed (II, 24, roll call 167) the resolution (from Stage 1) "to be chosen by the National Legislature" and it was in order to consider "for the term of seven years." Houstoun moved and Morris seconded to postpone, and the Convention approved. Then "to be ineligible a second time" came up in order, and Houstoun moved and Sherman (Conn.) seconded to strike it out. Morris argued that ineligibility "destroyed the great motive to good behavior," as if "saying to him, make hay while the sun shines." Ineligibility was struck out, 6-4 (II, 24, roll call 168), and "for a term of seven years" was again taken up, presumably pursuant to Houstoun's first motion. McClurg moved and Morris seconded to strike out "seven years" and insert "good behavior." This motion failed, 4-6 (II, 24, roll call 169), but it certainly stirred things up.

Implying, as it did, life tenure, it shocked McClurg's Virginia colleagues, especially since Washington was the only serious candidate. Mason sharply rebuked McClurg, prophesying that, if adopted, his motion would lead to hereditary monarchy and revolution. Madison, on the other hand, was embarrassed and felt obliged to defend McClurg by showing that his motion embodied the separation of powers, even though it might not be the "proper" expedient.[4] This was the first time Madison joined the separationists. It seems certain that respect for McClurg forced him to speak out.

The main effect, however, was that McClurg's motion embedded intrigue in the Virginia proposal: If legislative election meant repeated elections with intrigue at each one, then the only way out for those fearful of intrigue was life tenure with only one election. If the Morris-Houstoun-McClurg stratagem did nothing else, it forced a general reconsideration on 19 July.

Were Houstoun's and McClurg's motions consciously heresthetical? Madison thought that McClurg's motion was contrary to his true taste and thus wholly a maneuver. Certainly his motion was no threat of monarchy without the prior elimination of ineligibility, which is what Houstoun's motions accomplished. I infer, therefore, that McClurg's motion was part of a planned sequence that included Houstoun's as well. (Observe that the reversal of motions from term length first and ineligibility second to ineligibility first and term length second was necessary to delete ineligibility. Almost certainly, in regular order, with a long term adopted first, the Convention would not have deleted ineligibility. Hence Houstoun's motions were essential both to consider term length on its own merits—which may be all that Houstoun intended—and, as Morris probably intended, pave the way for McClurg's motion, which was entirely appropriate and made sense once it appeared likely that the term would be short and that there would be "intrigue" at each election.) Of course McClurg may simply have stumbled into Morris's plot, although it is equally likely that Morris may have exploited McClurg's innocence and Houstoun's simple intention to debate the length of term on its own merits. From the omnipresence of Morris in the whole event—seconding Houstoun's first motion and McClurg's, speaking for Houstoun's second motion and McClurg's—I suspect, without chance of verification, that Morris planned the whole sequence. (It is less likely Houstoun did, for he seldom participated, and Sherman seems to have happened into the scene out of an ideological preference (I, 68) for reelection as a reward.)

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[4] Adding a later footnote (doubtless to protect himself from a charge of monarchical sympathies), Madison explained that he spoke "to aid in parrying animadversions likely to fall on . . . Dr. McClurg, for whom J. M. has a particular regard and whose appointment to the Convention he had actively promoted. The Docr. though possessing talents of the highest order, was modest and unaccustomed to assert them in public debate" (II, 34).

Whoever may have been the master heresthetician, we know from Madison's footnote, inserted, it is true, after 1787, that he thought McClurg's motion was consciously heresthetic: "The probable object of this motion was merely to enforce the argument against the re-eligibility of the Executive" (II, 33). McClurg himself implied the same in the debate, saying that, after ineligibility was removed (by Houstoun's motion) the "only mode left for effecting" independence was tenure during good behavior (II, 36). Actually, the heresthetical intent was probably deeper: to stigmatize legislative election as necessitating either ineligibility or monarchy. In a contemporary footnote appended to the record of the vote on McClurg's motion, Madison sensed this deeper heresthetic: "This vote is not to be considered as any certain index of opinion [i.e., in favor of 'during good behavior' or monarchy], as a number in the affirmative [four states, New Jersey, Pennsylvania, Delaware, and Virginia] probably had it chiefly in view to alarm those attached to a dependence of the Executive on the Legislative, & thereby facilitate some final arrangements of a contrary tendency" (II, 36). Later he added that "the avowed friends" of "during good behavior" were no more than three or four. If so, given that at least ten delegates had to vote yea in the four supporting states, clearly at least six delegates—including, of course, Madison himself who was necessary for a yea vote in Virginia—voted contrary to their true tastes to bring about an advantageous parliamentary situation. They succeeded for their heresthetical tactic certainly "facilitated" "contrary arrangements."

Rhetorical and Heresthetical Success

Two days later the Convention reconsidered ineligibility. This discussion led to, and subsequent decisions revealed, the rhetorical success of associating legislative election with the single term. On 17 July the currently adopted method was "by the national legislature," and the Convention retained the seven-year term (4-6, II, 24, roll call 170). But on 19 July, after the method of electors had been adopted, the Convention rejected both ineligibility for a second term (2-8, II, 51, roll call 184) and a seven-year term (3-5, with two divided), finally settling on six years (II, 51, roll calls 185, 186). Presumably, with legislative election out, protections against intrigue were no longer needed. When, however, "by the national legislature" was reinstated on 24 July, both the seven-year term and ineligibility were also reinstated (7-3, with one absent, II, 118, roll call 224). The return of legislative election apparently required the return of the protections. And when the electoral college had been finally adopted, the Convention twice rejected, by large majorities, motions for a longer-than-four-year term, presumably because the college obviated the need for protection against intrigue (II, 520, roll calls 453, 454).

Rather complacently and perhaps slyly, Wilson, who was surely one of the architects of the separationists' rhetoric, observed: "It seems to be the unanimous sense that the Executive should not be appointed by the Legislature, unless he is rendered ineligible a 2d. time" and he used this association to urge popular election, which would allow second terms (II, 56, 19 July). Wilson was almost, but not quite, correct about unanimity. Delegates on both sides did agree that legislative election implied ineligibility for a second term:

Supporters of legislative election: Mason (Va.): II, 112, 119 ("a second election ought to be absolutely prohibited"); Randolph (Va.): II, 54-55; L. Martin (Md.): II, 52, 58, 101; Williamson (N.C.): II, 58, 100-101, 501; Rutledge (S.C.): II, 57; C. Pinckney (S.C.): II, 111-112; and Butler (S.C.), ultimately a separationist: II, 112.

Separationists: G. Morris (Pa.): II, 54, 500; McClurg (Va.): II, 33; Wilson (Pa.): II, 56, 501-502; Hamilton (N.Y.): II, 524; Gerry (Mass.): II, 57; II, 100-102, 112.

Not all the delegates were, at the time Wilson spoke of unanimity, possessed of his sense of it. Four New Englanders refused the association because they, like G. Morris, believed strongly in re-eligibility as a reward. Ellsworth (Conn.) specifically denied that ineligibility was a "natural consequence of his being elected by the Legislature" (II, 101), and King (Mass.) elaborated by quoting Sherman (Conn.) (I, 68) "that he who has proved himself most fit for an Office, ought not to be excluded from holding it" (II, 55). Strong (Mass.) offered a different argument when he "supposed that there would be no necessity, if the Executive should be appointed by the Legislature, to make him ineligible a 2d. time; as new elections of the Legislature will have intervened; and he will not depend for his 2d. appointment on the same sett of men as his first was recd. from" (II, 100). Strong's argument was probably not persuasive, however, because most framers worried about intrigue for future election, not intrigue in the past. By the end of the summer, even Sherman had accepted the entire rhetorical stance of the separationists. Originally he had thought that "an independence of the Executive on the supreme Legislative, was . . . the very essence of tyranny" and "he was against throwing out of office the man best qualified" (I, 68). But as a member of the Brearley committee, he defended the electoral college as a way "to get rid of ineligibility, which was attached to the mode of election by the Legislature, & to render the Executive independent of the Legislature" (II, 499). Surely this is a remarkable instance of persuasion. It was accomplished by rhetorically and heresthetically associating intrigue with legislative election and then by associating legislative election with no second term. But although Sherman's capitulation is especially striking, the previously cited roll calls at the end of Stage 2 (184, 185, 186, and 224) indicate that Sherman's reversal was not unique.

The beauty of this whole maneuver is that the separationists used the agreement they shared with the legislative electionists about intrigue to persuade the New Englanders of the relevance and danger of intrigue. Having done so, the separationists used the support of the New Englanders to eliminate entirely election by the legislature. Altogether, G. Morris, Wilson, and Madison deserve great credit for their rhetorical success, but mostly, I suppose, Morris, for he was a dominant figure both in debate and in committee.

Coalition with the Small State Interest

Brilliant as was the rhetorical success, it was not enough to win. The separationists also needed allies. So I now turn to an analysis of their alliances.

Once the separationists brought about reconsideration on 19 July, King (Mass.) revived the proposal for popularly chosen electors and Paterson (N.J.) suggested that electors be chosen on the ratio of one for the smallest state and three for the largest. Ellsworth (Conn.) put Paterson's suggestion as a motion to strike out "by the national legislature" and to insert "electors appointed by the Legislatures of the States in the following proportion: One person from each State whose numbers shall not exceed 100,000—*Two* from each of the others, whose numbers shall not exceed 300,000—and *Three* from each of the rest." Temporal considerations are important for understanding this motion. Three days earlier (16 July), the Convention had voted (roll call 156) for equal representation in the Senate, doubtless its most important decision because it ensured that the small states would stay in the Convention and that the Constitution would be written. The vote was extremely close and pitted the delegates of the small states against the main intellectual leadership from Pennsylvania, Virginia, and South Carolina. Paterson (N.J.) had been the floor leader of the small states—or so I infer from Madison's *Notes* (II, 18). Now, in his euphoria, he was, with Ellsworth, apparently intending to push the small state interest even further by providing a relatively large voice for them in choosing the executive.

There was a great difference on 19 July from 16 July, however. On the subject of equal representation in the Senate, Wilson (Pa.), Morris (Pa.), Madison (Va.), Mason (Va.), Randolph (Va.), and Rutledge (S.C.) had fought to the bitter end. Indeed, on 17 July, when the large-state cause was wholly lost, G. Morris opened daily business with a motion to reconsider roll call 156, but he did not even get a second. In a pathetic aside, Madison remarked: "It was probably approved by several members, who either despaired of success, or were apprehensive that the attempt would inflame the jealousies of the smaller States" (II, 25). Morris, Wilson, and Madison were politically resilient, however, and on 19 July they immediately allied their delegations with the small state bloc, presumably because that was the only way to beat legislative election. In Table 2 roll calls 156 on representation and 182 on electors are set forth for comparison. The main changes from 156 to 182 are that Virginia and Pennsylvania joined the small-state side and North Caroina left it for the legislative election bloc. This must have been easy for North Carolina. Only four delegates were present, and only three voted yea on 156. Hence only two were necessary to join Spaight for the nay vote on 182. Williamson spoke against 182, so a nay on 182 needed only the conversion of the totally silent A. Martin or the almost silent Davie.

Unfortunately for the separationists, the small state-separationist coalition was disrupted by the interests of distant states. Butler (S.C.) (II, 59), Williamson (N.C.) (II, 59, 526), Spaight (N.C.) (II, 95, 99, 526), and Houstoun (Ga.) (II, 95 99) complained about the cost of sending electors to a capital in the Middle Atlantic states. An odd concern, so it seems today. Gerry (Mass.) thought so too (II, 100). But both New Hampshire and Georgia had difficulty that summer in financing their delegates. So on 23 and 24 July Houstoun moved and Spaight seconded the reinstatement of legislative election. The Convention approved (7-4, II, 98, roll call 215). As is apparent from Table 2, five of the seven states that voted yea are geographically peripheral. New Jersey and Delaware are, however, pivotal in the change from 182 to 215, and I can only guess why. Three New Jersey delegates were present in late July (Brearley, Livingston, and Paterson, III, 588; IV, 72), and the delegation did not have the required three to vote on 18, 21, and 23 July. My guess is that the delegation was split 2-1 on the method of election, and one person changed sides on vote 215, perhaps in deference to peripheral states. As for Delaware, no explanatory evidence exists, although I suspect, from its otherwise erratic voting, the same kind of absence and individual change as in New Jersey.

The reinstatement of legislative election did not, however, settle the issue. The separationists and others continued to offer alternatives. Wilson (Pa.) proposed a method of lot, but this was clearly heresthetic to emphasize his hostility to legislative election, and he seconded a motion to postpone (II, 105-106). Ellsworth (Conn.) offered a compromise: legislative election, except when an incumbent was re-eligible, in which case electors would elect. This failed 4-7 on roll call 218 (Table 2), with the previous blocs splitting up in curious ways. C. Pinckney (S.C.) offered a motion restricting the executive to six years' service out of any twelve. This too may have been intended as a compromise, but it failed (II, 108, roll calls 219 and 220). On 26 July Mason spoke powerfully for legislative election and moved reinstatement of "seven years" and "ineligible a second time." This passed 7-3 (roll call 224) and was followed immediately by reaffirmation of the whole resolution 6-3, with one divided and one absent (roll call 225). It is not hard to explain roll calls 224 and 225. The majority was made up of states devoted to legislative election, peripheral states, and New Jersey with its wobbly and knife-edge majority. Massachusetts was divided or evasive, as on most of these votes. Virginia, however, shifted from separationist to legislative election for easily explicable reasons: Its separationist majority on votes 182 and 215 doubtless consisted of Madison, McClurg, Washington, and Blair against Mason and Randolph. But McClurg went home before

Table 2. Indecision at the End of Stage 2

Date and Stage	Roll Call Number and Page	Movers	Motion	Outcome	Yea	Nay	Divided
16 July Stage 2	156 II, 15		To accept the report for equal representation in upper house	5-4-1 (N.H., N.Y. absent)	Conn., N.J., Del., Md., N.C. (small state bloc)	Pa., Va., S.C., Ga. (large state bloc)	Mass.
19 July Stage 2	182 II, 51	Ellsworth	To strike out "by the national legislature" and insert "by electors in the ratio of. . ."	6-3-1 (N.H., N.Y. absent)	Conn., N.J., Pa., Del., Md., Va. (small state and separationist bloc)	N.C., S.C., Ga. (legislative election bloc)	Mass.
24 July Stage 2	215 II, 98	Houstoun Spaight	To strike out "by electors" and insert "by the national legislature"	7-4 (N.Y. absent)	N.H., Mass., N.J., Del., N.C., S.C., Ga. (distant states and legislative election bloc)	Conn., Pa., Md., Va. (small states and separationists)	
24 July Stage 2	218 II, 108	Ellsworth	To add "except when the magistrate last chosen . . . in which case the choice shall be by electors"	4-7 (N.Y. absent)	N.H., Conn., Pa., Md.	Mass., N.J., Del., Va., N.C., S.C., Ga.	
26 July Stage 2	224 II, 118	Mason	To insert "for a term of seven years, to be ineligible a second time"	7-3 (Mass., N.Y. absent)	N.H., N.J., Md., Va., N.C., S.C., Ga.	Conn., Pa., Del.	
26 July Stage 2	225 II, 118		To agree to the resolution on election of the executive	6-3-1 (Mass., N.Y. absent)	N.H., Conn., N.J., N.C., S.C., Ga.	Pa., Del., Md.	Va.

the votes of 26 July, and Blair switched between votes 215 and 225. (Blair was the swing vote in Virginia, and we know almost nothing else about him. Of the six Virginia votes for which Madison records the detail, Randolph and Mason were always opposed to Madison. Washington joined Madison four times out of five, and McClurg joined him two out of two. But Blair sided with Randolph and Mason thrice and with Madison thrice.)

Thus ended Stage 2 with a victory for legislative election, although the separationists had raised considerable doubt about its propriety.

Revelation of a Cycle

In Stage 3, on 24 August, when the Convention again took up the method of election, Rutledge (S.C.) immediately moved to elect by joint ballot of the two houses. This was great luck for the separationists. Rutledge had been a diehard opponent of equal representation in the Senate (roll call 156, Table 2), and this motion was doubtless an oblique counterattack to regain part of what the large states had lost on 16 July.

The New Jersey and Connecticut delegates responded rigorously, playing the role of leaders of the small states—as they had on the issue of representation in the Senate—even though by 24 August the original captains, Paterson and Ellsworth, had gone home. Sherman (Conn.) objected that a joint ballot would deprive the "*States* represented in the *Senate* of the negative intended them in that house" (II, 401); and Dayton (N.J.) said he "could never agree" because a "joint ballot would in fact give the appointment to one house." Brearley (N.J.) supported them and Wilson (Pa.), Madison (Va.), and Gorham (Mass.) defended Rutledge. Here flared up, with immediate intensity, a reflection of the great dispute on representation: Pennsylvania, Virginia, Massachusetts, and South Carolina against Connecticut and New Jersey. When put to a vote, Rutledge's motion passed 7-4 (roll call 356, Table 3). Then Dayton moved that, on the joint ballot, each state have one vote, which failed 5-6 (roll call 357, Table 3). Note also in Table 3 that Dayton reactivated the small state bloc of 16 July. (On roll call 356 Delaware inexplicably voted against its interest, but on 357 it had rejoined the bloc. Only New Hampshire, not present on 16 July, voted with the big states, which Langdon (N.H.) explained was owing to its experience of deadlock between the Houses in gubernatorial elections.)

G. Morris, ever the opportunist and an exceptionally adroit parliament man, took the chance to reactivate the coalition of small states and separationists. He moved for a combination of the electors espoused by small states and the popular election espoused by separationists. Morris lost (roll call 359, Table 3), but he reactivated the coalition he and Ellsworth had formed in Stage 2. Pennsylvania and Virginia—presumably Madison had won back Blair—joined Connecticut, New Jersey, and Delaware. Only Maryland is

Table 3. The Approach to the Committee on Postponed Matters: Stage 3

Date	Roll Call Number and Page	Movers	Motion	Outcome	Yea	Nay	Divided
24 August	356 II, 399	Rutledge	To insert "joint" before "ballot"	7-4 (N.Y. absent)	N.H., Mass., Pa., Del., Va., N.C., S.C.	Conn., N.J., Md., Ga.	
24 August	357 II, 399	Dayton Brearley	To insert "each State having one vote"	5-6 (N.Y. absent)	Conn., N.J., Del., Md., Ga.	N.H., Mass., Pa., Va., N.C., S.C.	
24 August	359 II, 399	G. Morris Carroll	To be elected by electors chosen by the people	5-6 (N.Y. absent)	Conn., N.J., Pa., Del., Va.	N.H., Mass., Md., N.C., S.C., Ga.	
24 August	360 II, 399	Broome	To commit last two clauses of Article X, section 1	5-5-1 (N.Y. absent)	N.J., Pa., Del., Md., Va.	N.H., Mass., N.C., S.C., Ga.	Conn.
24 August	361 II, 399	G. Morris	To be elected by electors	4-4-2 (N.Y., Mass. absent)	N.J., Pa., Del., Va.	N.H., N.C., S.C., Ga.	Conn., Md.

out of place. (I think Jenifer left the room: on 356 and 357 at least three Maryland delegates voted with the small states. On 359—if Jenifer was out —McHenry and Martin probably outvoted Carroll. On 360 Maryland was back with the small states, and on 361 Maryland divided, perhaps with Carroll and Jenifer together.)

Then Dayton (N.J.) moved to postpone, which failed by voice vote, and Broome (Del.) moved to refer to a committee, which failed in a tie (roll call 360, Table 3), with Maryland back in place and Connecticut inexplicably divided. Doubtless encouraged by the tie vote on 360, Morris moved for electors without specifying how they would be chosen. This too resulted in a tie (roll call 361, Table 3). The Convention, now warned of deadlocks, postponed consideration and later consigned the issue to a committee on postponed matters (II, 463, 31 August), which invented the electoral college.

The route for reopening the issue was the revelation of a voting cycle. Let: *a* stand for "legislative election without joint ballot," *b* stand for "election by electors," *c* stand for "legislative election with joint ballot." Then observe:

a beat *b,* in roll calls 215 and 225 (Table 2), Stage 2,
b tied with *c,* in roll call 361 (Table 3), Stage 3,
c beat *a,* in roll call 356 (Table 3), Stage 3.

Had Rutledge not brought up the joint ballot, this cycle would not have been revealed—indeed, would not have existed. Legislative election would probably have survived. Once revealed, however, the cycle appeared as a deadlock, which is the way cycles are usually interpreted by those who do not know about social choice theory. However interpreted, the significance of the cycle that Morris revealed was that it gave him another chance in the committee.

Committee on Postponed Matters

The delegates elected to the Committee on Postponed Matters were almost entirely separationists or their allies.[5] G. Morris (Pa.) and Madison (Va.) were the most vocal separationists. King (Mass.), Dickinson (Del.), and Carroll (Md.) had usually supported them, when others from their states had not. Sherman (Conn.) and Brearley (N.J., the chairman) were at this time the spokesmen for the small state interest. As for the others, for each state the one elected was the one most likely to support the coalition: Gilman (N.H.) rather than Langdon, who had spoken for the joint ballot; Baldwin (Ga.) rather than Few, who never uttered a word, according to Madison's *Notes;* Butler (S.C.), who had spoken of "intrigue" rather than Rutledge or C. Pinckney, highly vocal supporters of legislative election; and Williamson (N.C.) who feared "intrigue," rather than Spaight, who seconded Houstoun. Thus the separationist-small state coalition had at least seven sympathizers, and perhaps ten, because Baldwin, Gilman, and possibly Butler voted for the committee report.[6] Only Williamson persisted in opposition.

As a result the committee tailored a plan to satisfy all those who might oppose legislative election. For distant states, electors were to meet in the states, thereby saving a trip to the capital. For those in favor of popular election, electors were to be chosen in the manner prescribed by state legislatures, which allowed for popular election. For the separationists, the college avoided the legislature entirely, if any candidate got a majority. For the small state interest, there were two provisions: First, each state was to have as many electors as Representatives and Senators, which gave the small states an edge. Second, if no candidate had a majority of electoral votes, the Senate (wherein states were equal) was to choose from the five highest.

It is uncertain just what the framers expected about election in the Senate. Some separationists accurately forecast that one candidate would usually get an electoral majority (G. Morris, II, 512), because, as Madison pointed out, it was to the advantage of large states to avoid decision in the Senate (II, 513) and because, as Baldwin pointed out, "increasing intercourse . . . would render important characters less and less unknown" (II, 501). On the other hand, some supporters of the committee plan (Sherman, II, 512-513; King, II, 514) apparently believed the Senate would usually elect, perhaps as often as "nineteen times out of twenty," as legislative electionists like Mason (II, 512) and C. Pinckney (II, 511) believed. On balance, most people anticipated just what would benefit them, a systematic bias that rendered the plan almost universally appealing.

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[5] Probably the delegates voted individually rather than by states in electing committees, thereby giving the large delegation from Pennsylvania an advantage with which it significantly affected the outcome here.

[6] Georgia, with two delegates, voted yea, so Baldwin did. New Hampshire, with two delegates once voted for (roll call 457, II, 520) and once was divided (roll call 445, II, 508), from which I infer Gilman voted yea against Langdon and then converted him. South Carolina voted nay; but if C. C. Pinckney joined Rutledge and C. Pinckney, then Butler could have voted yea without a trace.

Consequently, the plan survived on the floor without substantial emendation and was twice approved by a large majority: (a) on a motion by Rutledge to postpone to take up legislative appointment (roll call 445, II, 508), which failed 2-8-1, with New Hampshire divided and the Carolinas nay; (b) on the report of the committee (roll call 457, II, 520), which passed 9-2, with the Carolinas dissenting.

One change was adopted on the floor: to relocate the residual power to elect from the Senate to the House, voting by states. This preserved the advantage for the small states and satisfied some who thought the Senate might be too powerful. Otherwise the committee plan stuck. It is astonishing that a compromise put together over a weekend to satisfy diverse, parochial, and temporary interests has, with only slight modification by the Twelfth Amendment, served adequately for two centuries.

We know nothing for certain about how this compromise was made, but I infer that G. Morris put it together. He was an active and dominating floor leader,[7] whose colleagues respected his ability,[8] who creatively suggested deals,[9] and who knew how to abandon hopeless positions.[10] So he was precisely the kind of person to take charge of a committee. Furthermore, on the floor he acted as committee spokesman, giving "the reasons of the Committee and his own" (II, 500). Even more suggestive, however, is his response to Wilson's criticism of the plan for ultimate election in the Senate. Through Madison's emotionless precis, one still senses Morris's sharp resentment, as a proud and offended author, of the remarks of a colleague from whom he expected support: "Mr. Govr. Morris expressed his wonder at the observations of Mr. Wilson so far as they preferred the plan in the printed Report [i.e. of the Committee on Detail] to the new modification of it before the House, and entered into a comparative view of the two, with an eye to the nature of Mr. Wilson's objections" (II, 523).

But although we can only guess at Morris's role, we know that the report took the supporters of legislative election by surprise. Randolph (Va.) and C. Pinckney (S.C.) "wished for a particular explanation . . . of the reasons for changing the mode" (II, 500). Morris responded. But what he could not tell them were facts he probably did not fully understand himself: that the Virginia proposal was about to be finally replaced because of the separationists' rhetorical and heresthetical skill and persistence, because of the cycle generated by Rutledge's unwise motion, and because of the clever appeal to diverse interests put together in the proposal for the electoral college.

Generalizations about Heresthetics

I undertook this interpretation of the origin of the electoral college in order to explicate political invention and the relation between creativity and social regularities. Now I extract the moral from my story.

In the static model of decision making, choice is made from a fixed set of alternatives. The dynamic process differs. The set of alternatives is not fixed, and the alternatives themselves change over time. In my example, only one alternative existed initially, many were invented during the course of "narrowing the alternatives," and the winning one was not created until most others were disposed of. Furthermore, *all* the continuing alternatives were changed in gross or subtle ways throughout the event. As shown in Table 1, the motion for popular election was never incarnated in the same words twice. The system of electors adopted on 19 July differed much from the college adopted on 6 September. Even the Virginia proposal was transformed. When Rutledge offered it against the electoral college on 5 September, it was changed by his idea of a joint ballot from simply a method of election, as it had been on 23 June or 26 July, to an expression of the power of the large states.

In my example, and probably in general, the set of alternatives is indefinitely large, and the continuing elements are constantly revised. This continuous creation is, of course, the artistic element of the dynamic world that differentiates it from the static model.

The setting for creation in this example was the composite of the desire to win and the expectation of losing. The separationists' intense will to win was demonstrated by their persistent invention of new forms of popular election and by their readiness to ally with the small state interest, even though they had lost to that interest on something much more important and painful. Their expectation of losing on the presidency was demonstrated by the large majorities by which legislative election was adopted on roll calls 215 and 225 (Table 3). Even the one (disastrous) creation by the supporters of legislative election (namely, Rutledge's motion for a joint ballot) was immediately interpreted, not so much as a motion on the method of election (on which Rutledge was winning), but as a motion on the composition and authority of the Senate (on which Rutledge had lost). So creativity on both sides emanated from the will to win in the face of prospective loss.

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[7] By Madison, *Notes,* we know he spoke more frequently than anyone else (Rossiter, p. 252).

[8] They chose him for committees more frequently, relative to expectations, than any other active delegate. (For him, the ratio of actual to expected committee membership was 2.8, for King 2.5, for Rutledge and Williamson 2.3.)

[9] He suggested the compromise on the slave trade and navigation laws (II, 374).

[10] In the debate on impeachment, he initially opposed it on separationist grounds as just another chance for "intrigue," but when Davie (N.C.), Wilson (Pa.), Mason (Va.), Franklin (Pa.), Madison (Va.), Gerry (Mass.), and Randolph (Va.) favored it and only two young men, C. Pinckney (S.C.) and King (Mass.), supported it, Morris quickly acknowledged his "opinion had been changed" (II, 64-68).

I believe this motivation, so clear in my example, is probably quite general. The political world selects for people who want to win politically; that is, those who do not want to win are more likely than others to lose and thus be excluded from political decisions. (In my example McClurg, whom Madison thought something of an innocent, was so taken aback by the adverse reaction to his one heresthetical effort that he went sulkily home (II, 67).) Consequently, most of the people—and certainly most of the leaders—in the political system display the same kind of persistence as did G. Morris, Madison, Sherman, Rutledge, and others. In the Philadelphia Convention this persistence was layers deep: after Paterson and Ellsworth went home, Brearley and Dayton were just as determined in the small states' cause as their more significant colleagues had been. This more or less general motivation is, so I believe, what makes generalization about politics possible. Most participants have the same goal, namely, to win on whatever is the point at issue. Assuming they think seriously about how to achieve their goals, they may be expected to behave in similar ways.

This characteristic of political actors is the basis for the reconciliation, by rational choice analysis, between determinism and indeterminism. The combination of an open-ended set of alternatives with the presence of people motivated to win makes possible many generalizations about the process of winning. Both the size principle and the median voter principle, for example, depend on exactly these conditions: participants who are motivated to win and who creatively adjust alternatives to arrive at minimal winning coalitions (Dodd, 1976; Enelow & Hinich, in press; McKelvey, 1976, 1979; Riker, 1963; Riker & Ordeshook, 1972, chap. 11-12). So also other recent generalizations about political dynamics depend on these conditions (Kramer, 1977; McKelvey, Ordeshook, & Winer, 1978).

It seems to me that many more such generalizations are possible, which is why I recommend the study of heresthetics. We know, for example, almost nothing about the way alternatives are modified in political conflicts. Yet this kind of heresthetical maneuver is how groups are forced into minimal winning coalitions and coalitions aimed at the median voter. In my example the separationists adopted a rhetorical stance and hammered at it until they were successful. They tested out alternatives, rejecting or exploiting them as appropriate. I suspect there are patterns of such behavior. Perhaps there are regularities in the way rhetorical positions are established, appeals, for example, to well-established references and symbols, like the separationists' appeal to well-established and hallowed principle (i.e., the separation of powers doctrine) and to a well-established argument (i.e., about the danger of intrigue in legislative election). Perhaps there are regularities in the way losers coalesce with defecting subsets of apparent winners, as separationists allied with the small state interest. Perhaps even there are regularities in the way apparent losers develop cycles and exploit them when revealed, as Morris developed and exploited a cycle out of Rutledge's motion for a joint ballot. The evidence from an anecdote like the one I have related is not, of course, enough to establish any generalization. But to me the evidence suggests there are underlying patterns which are worth looking for. There is no chance they will turn out to be a general equilibrium (Riker, 1980), but they may be partial equilibria or at least involve some repetitive events. And so, as retiring president, I commend this search to you.

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Changes in the Vote Margins for Congressional Candidates: A Specification of Historical Trends

JAMES C. GARAND
Georgia State University

DONALD A. GROSS
University of Kentucky

Recent research on House elections has focused on the decline in congressional competition since the mid-1960s. However, this body of research suffers from three major problems: 1) an imprecise specification of the nature of change in the degree of congressional competition, 2) its exclusive reliance on a limited time frame, which calls into question the generalizability and adequacy of its explanations, and 3) its overemphasis on incumbency-oriented explanations. Using a Multiple Interrupted Time Series (MITS) analysis of trends in aggregate electoral data from 1824 to 1980, we find that current low levels of congressional competition are not historically unique, but instead constitute a continuation of a long-term trend established in the mid-1890s. In addition, and in contrast to the general thrust described in the literature, there appears to be a post-1965 trend toward greater competition, although the absolute level of competition is lower after the mid-1960s. Finally, a disaggregation of electoral results by incumbency status suggests a growth in the incumbency advantage since the mid-1890s, but that lower post-1965 levels of congressional competition are the result of a winners' and not an incumbency, advantage. Such findings cast doubt on the adequacy of explanations that focus on post-1965, incumbency-oriented factors.

The decine of competition in congressional elections during the 1960s and 1970s has been a widely recognized contemporary development. In recent years a large number of analyses have been undertaken in an attempt to explain this phenomenon (Alford & Hibbing, 1981; Born, 1977, 1979; Collie, 1981; Erikson, 1971, 1976; Ferejohn, 1977; Fiorina, 1977a, b; Gross & Garand, 1982; Mayhew, 1974). Four types of explanations have dominated the literature: redistricting (Tufte, 1975); a growth of incumbency resources (Fiorina, 1977a, b); weaker opponents (Hinckley, 1981; Jacobson, 1981; Mann & Wolfinger, 1981); and changes in the electorate (Burnham, 1970; Ferejohn, 1977).

Given the volume of literature on the subject, it is surprising to find that so few analyses have attempted to define the precise nature of recent developments in congressional competition. Although Erikson (1971) and Alford and Hibbing (1981) established that the increase in electoral margins was uniform across incumbency levels, it is not clear that only incumbents have increased their electoral margins since 1965. Nor is it clear how post-World War II developments in congressional competition compare with those of earlier years. Thus, what may seem to be a major development in the 1960s might seem to be only a minor development when compared to earlier, more significant and long-lasting changes. Finally, the precise nature of changes in the degree of competition, whether from a long-term or short-term perspective, remains unspecified. In this article we consider each of these three problems. By developing a detailed description of long-term historical trends in congressional competition, we hope to gain further insight into the applicability of various explanations of cross-temporal variation in congressional election outcomes.

Evaluating Change in Congressional Competition

As a point of departure it is important to consider alternative representations of change in congressional competition over an extended time series. As such, Figure 1 illustrates four hypothetical examples of change in the mean percentage of the vote obtained by winning congressional candidates over the period from 1946 to 1980. Since 1965 is often seen as a turning point in congressional competition, we can divide the entire time series into two periods of theoretical importance:

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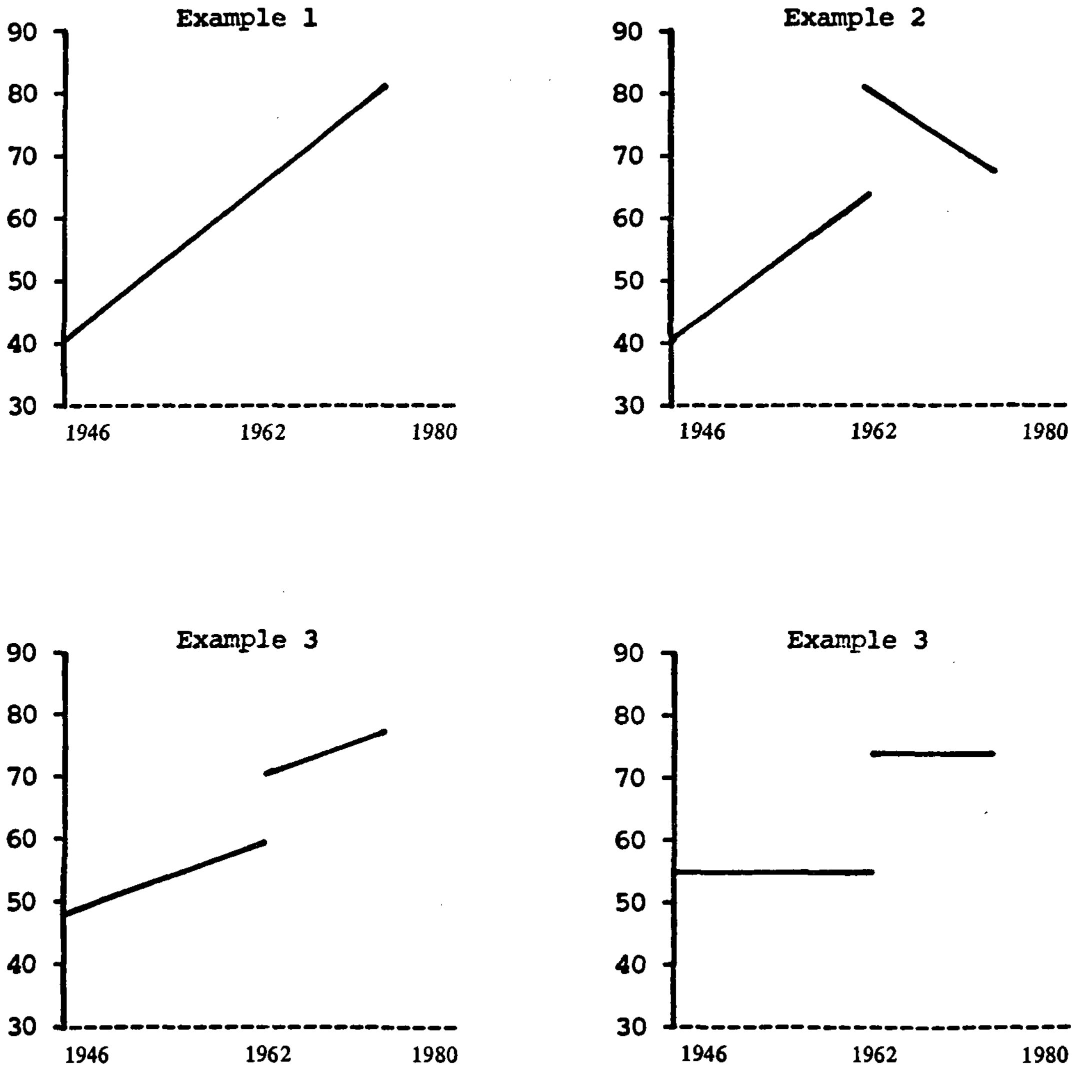
1946 to 1964 and 1966 to 1980. In each of these four examples the mean percentages during the first half of the time series are equal ($\bar{X} = 55$), as are the mean percentages for the second half of the time series ($\bar{X} = 73$). Hence, in each of these four hypothetical examples the mean vote is 18% higher in the second half of the time series than in the first half.

In the first example there is a pure incremental process of change across the entire time series; that is, the rate of change from every t to every $t+1$ remains constant. Example 2 illustrates a situation in which one rate of change operates in the period from 1946 to 1964 and another rate of change operates in the period from 1966 to 1980. Thus, it can be said that there is a change in the incremental process of change from one period to the next. In Example 3 the process of change is the same in both temporal periods; however, the same process operates at a different level in each period. Finally, Example 4 again illustrates a situation in which a similar incremental process operates at different levels in each period. Unlike Example 3, however, the incremental process operates such that there is not a process of systematic change within each period—i.e., the dif-

Figure 1. Four Hypothetical Examples of Change in the Mean Proportion of the Vote for Winning Congressional Candidates, 1946-1980



ference between each t and each $t+1$ is equal to zero within each period.[1]

In terms of our understanding of recent developments in congressional competition, each of these examples should focus our attention upon different phenomena to be explained and different types of explanations. For Example 1, the theoretical utility of distinguishing between the pre- and post-1965 periods would be greatly suspect, as would all explanations based primarily on such a distinction. The phenomenon of greatest interest is the ongoing constant process of change over the entire time series, with the greatest need being explanations that focus upon this process. Example 2 requires explanations of the process of change occurring within each temporal period and the reason why the process is different in each period. The constant process of change and the causes of the observed level shift are the phenomena of interest in Example 3. Since there is no process change in Example 4, analyses of change in congressional competition need only focus on factors that may account for the level shift. Although these examples illustrate the need to describe precisely the nature of change in congressional competition, it is important to recognize that numerous combinations of process and level changes are possible for a given time series.[2]

Not only have analysts failed to distinguish among alternative representations of change in congressional competition, most analysts have examined only a fairly limited time frame: 1946 to 1980. Reliance upon such a limited time series can result in three major problems. First, it may be difficult to distinguish among alternative representations of change in the time series. Changes in the level of the time series may be confused with process changes, and vice versa. Second, what may appear to be a significant and major change in the short run can appear as an insignificant or minor change from the perspective of a longer time series. Finally, as is suggested by Cooper and Brady (1981), the reliance upon relatively short time series prevents the political scientist from utilizing history as a laboratory—a testing ground, if you will—for theories offered in temporally constrained analyses.

Having discussed the need to describe precisely the nature of change in congressional competition over a more extensive time series, we must address one final consideration: can changes in congressional competition be attributed to changes in congressional elections that involve particular types of candidates? Most analyses have attributed recent declines in congressional competition to changes in elections that involve incumbents. On the other hand, it is not clear that the trend toward increased electoral margins is confined to incumbent candidates. Data presented by Collie (1981) suggest that others—i.e., winners in open-seat elections—have also benefitted from post-1965 changes.[3] Thus it is important to consider the likelihood that there are changes in the electoral margins of particular types of nonincumbents. However, it is inappropriate merely to compare all nonincumbents with all incumbents. The essential problem with such a comparison is that all incumbents can be considered, almost by definition, as viable candidates, whereas a large number of nonincumbents are not viable candidates.[4] (Incumbents have shown that they are viable by winning in at least one previous election.) Nonincumbents are much more heterogeneous in terms of resources and electoral viability, with any subsequent calculations affected by this heterogeneity. It is important, therefore, to reduce the heterogeneity among nonincumbents and compare only viable nonincumbents with incumbents. Since electoral viability is difficult to define empirically and precisely, the easiest, and we feel best, way to undertake comparisons is to compare winning nonincumbents with winning incumbents. We may not be able to say anything about various losing candidates, but we do know that all individuals in both winning categories are viable candidates.

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[1] Although there are important theoretical differences between example 3 and example 4, from a methodological perspective the latter is merely a special case of the former, where change within each period is set equal to zero.

[2] The methodologies employed in many previous analyses cannot distinguish among various representations of change; in fact, they mask such differences. For example, Alford and Hibbing (1981) compared the mean of the scores for one time period with the mean for the second time period. All four examples in Figure 1 would give the same results when the means for each period are compared.

[3] Extrapolating from the data given in Table 1 of Collie's (1981) article, the mean percentage of open seat winners with safe victory margins from 1952 to 1964 is 32.24, whereas the mean is 38.88 for the period of 1966 to 1976. An analysis by Gross and Garand (1982) also indicated that nonincumbent winners have achieved greater electoral margins after 1964.

[4] It is difficult to define precisely what one means by a viable candidate. It may be suggested that one can solve the problem of weak candidates only by dealing with contested elections. Ignoring the problem of defining contested elections, we do not feel that focusing exclusively on contested elections is an adequate solution to the problem. One would still have a large number of nonincumbent candidates who are, for all practical purposes, very weak opponents. It is also important to recognize that in any given election an incumbent, because of scandal or various other reasons, may not be a viable candidate.

Having now defined the need for greater precision in the description of changes in congressional competition, we can begin by describing the nature of changes in congressional competition for the period from 1824 to 1980.

Trends in Congressional Competition over Time

Before we begin to describe trends in competition for congressional elections over the period from 1824 to 1980, a discussion of the data used in this analysis is in order. General election results and data on candidate attributes for all congressional candidates were used to create several important candidate variables: North vs. South, winner vs. loser, and incumbency status (nonincumbent, first-term incumbent, and multiple-term incumbent).[5] Only the top two candidates in each election were selected for inclusion in this study, with each candidate's proportion of the vote defined in terms of the proportion of the two-party vote. Thus minor party candidates could be removed from the analysis with little impact on each election. In addition, mean proportions of the two-party vote were computed for each congressional election year during the period under study and for five sets of congressional candidates: all winners, nonincumbent winners, first-term incumbent winners, multiple-term incumbent winners, and all incumbent winners.

The trend in the average proportion of the two-party vote going to northern winning congressional candidates is shown in Figure 2. Since competition is highest when two competing candidates hold roughly equal electoral support, this figure can be seen as representing the reciprocal of congressional competition: when the vote proportions for winning candidates are high, competition is low, and vice versa. Several important features of this trend stand out. First, the vote proportions of northern winning candidates exhibited a generally negative trend from 1824 to 1894, then turned sharply upward and followed an upward trend from 1896 to 1930. After a sharp decline in the vote proportions in 1932, the trend again began to follow a positively sloped line until 1965, when the time series shifted up to a higher level but underwent a significant dampening out of the slope parameter.

Second, the current high proportion of the vote for winning candidates (and associated decline of competition) is not a unique contemporary development, given the equally high vote proportions existing during the 1920s. Recent trends in winners' vote proportions appear to be consistent with a long-term trend beginning in 1932, and consistent with an even longer-term upward trend beginning in 1896. It would appear that the contemporary high level of vote proportions for winning congressional candidates is not unique, given the equally high vote proportions existing during the 1920s. One might argue that explanations of recent patterns in congressional competition during the 1960s should encompass the similar trend occurring since the turn of the century and, more specifically, the low levels of congressional competition found in the 1920s.

Third, it appears that shifts in the trend of congressional competition are closely tied to periods that are viewed as critical realigning periods (i.e., the early 1850s, the early 1890s, and the early 1930s). Before the election of 1860, a downward trend in winners' vote proportions was broken temporarily in 1854 by a moderate rise in the trend line, after which the downward trend resumed until the early 1890s. The election of 1894 was followed by a steep positive trend in the vote proportions of winning congressional candidates, broken temporarily by a sharp drop in 1932, after which a positive slope to the trend line resumed.

A more rigorous specification of patterns of congressional competition can be achieved through the application of multiple interrupted time series (MITS) analysis to the plot of vote proportions of winning congressional candidates over

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[5] The data utilized in this article were made available in part by the Inter-University Consortium for Political and Social Research. Neither the original collectors of the data nor the Consortium bear any responsibility for the analysis or interpretation presented herein. Because of data problems, approximately 2 to 3% of the congressional candidates in any given year were misclassified in terms of their incumbency status. Southern states excluded from the analysis were Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Virginia, Kentucky, Maryland, Oklahoma, Tennessee, and West Virginia. Like all previous analyses of electoral competition in congressional elections, ours excludes Southern states from consideration because of the unique political history of the region. For most of the post-Civil War era Southern congressional elections were decidedly one-sided, with Southern congressmen winning by extreme margins and maintaining high levels of tenure and electoral safety. Beginning in the 1960s the "solid South" began to break down, and Southern congressional elections began to become more competitive. The inclusion of Southern congressional elections in our national totals could, therefore, mask important national trends with the unique political history of the region. For the purposes of this study, we follow the lead of Alford and Hibbing (1981) by concentrating on northern states only, although our set of southern states is slightly more inclusive than that employed by these authors.

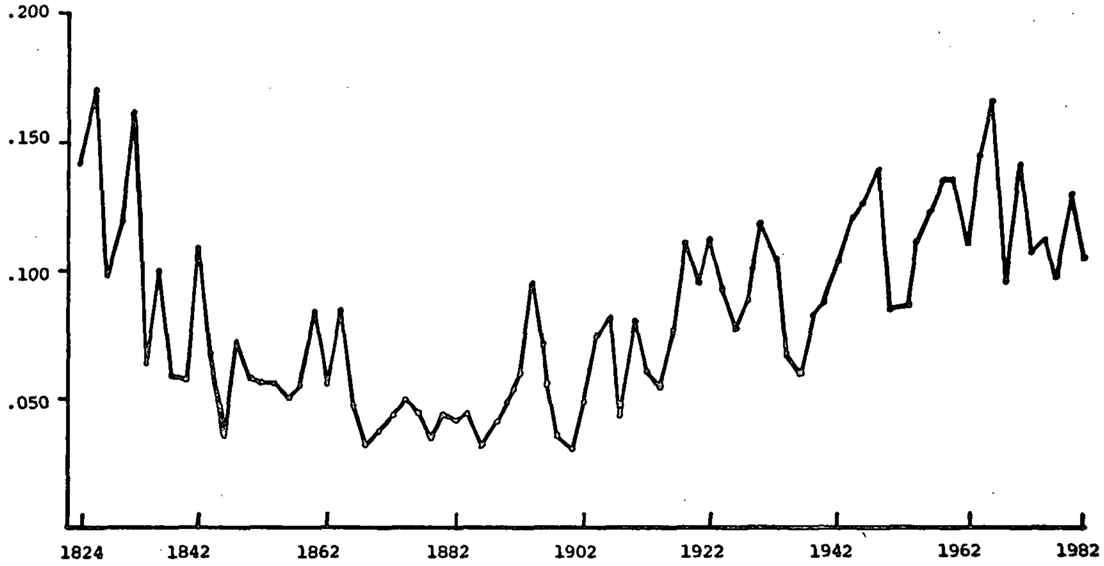

Figure 2. Mean Proportion of the Two Party Vote for Northern Winning Congressional Candidates, 1824-1980

time.[6] Since shifts in the time series appear to occur during the theoretically relevant periods of realignment, specific elections occurring during these periods are utilized as interventions in the time series.[7] These interventions were specified in the following regression equation:

$$\begin{aligned} WIN_t = {} & b_0 + b_1 COUNTER_t \\ & + b_2 DUMMY854_t + b_3 COUNT854_t \\ & + b_4 DUMMY894_t + b_5 COUNT894_t \\ & + b_6 DUMMY932_t + b_7 COUNT932_t \\ & + b_8 DUMMY966_t + b_9 COUNT966_t \\ & + e_t. \end{aligned} \tag{1}$$

[6] MITS is a statistical procedure utilized primarily in policy studies to analyze and specify the impact of some set of interventions over an extended time series (Albritton, 1979; Campbell & Stanley, 1966; Cook & Campbell, 1979; Hibbs, 1977; Lewis-Beck, 1979; Lewis-Beck & Alford, 1980; McDowell et al., 1981). It allows the researcher to assess the degree to which the parameter values (slope and intercept) of the trend shift from one time period to the next. MITS is conceptually equivalent to running separate regressions for each time period and determining the similarity of parameter estimates (or values) for the time period before and after some intervention. A series of dummy variables (representing shifts in the intercept) and counter variables (coded 0 before and 1, 2, 3, . . . , *N* after an intervention, and representing shifts in the slope) are introduced into the time series regression equation; significant parameter estimates for these dummy and counter variables indicate significant shifts in the intercept and slope, respectively, of the regression associated with the hypothesized intervention. For an excellent application of MITS see Lewis-Beck and Alford (1980).

[7] Interventions utilized in the MITS analysis were seen as occurring in 1854, 1894, 1932, and 1965. With the exception of the latter intervention, these time points were determined primarily on the basis of general agreement about periods of partisan realignment found in the literature. Although there has been extensive debate about the occurrence of a partisan realignment in the 1960s, the 1965 intervention point was included in the analysis because of the importance attributed to the post-1965 period by scholars of congressional elections such as Fiorina (1977a, b).

where WIN_t = average annual proportion of the vote captured by winning congressional candidates; $COUNTER_t$ = a counter for each election year from 1824 to 1980, coded from 1 (1824) to 79 (1980); $DUMMYyyy_t$ = a dummy variable coded 0 before year *yyy* and 1 for *yyy* and after; $COUNTyyy_t$ = a counter for election years, coded 0 before *yyy* and 1, 2, 3, . . . for *yyy* and after; b_0, b_1, b_2, b_3, b_4, b_5, b_6, b_7, b_8, and b_9 are parameters to be estimated; and e_t is a randomly distributed error for each election year t. Significant parameter estimates obtained for any of the intervention dummy variables indicate a statistically significant shift in the regression intercept associated with the respective intervention point, and significant parameter estimates for any of the counter variables (excluding *COUNTER*) indicate a statistically significant shift in the slope.[8] In addition, the actual slope coefficient for each time period can be obtained by summing the counter coefficients associated with earlier time periods.

The results of the MITS analysis for winners' vote proportions over time are shown in Table 1.[9] These results indicate a clearly specified pattern of congressional competition from 1824 to 1980 which is compatible with the general description of the trend offered above. The time series begins with a negative slope (indicating a trend toward greater competition) from 1824 to 1852 and then undergoes a significant positive shift in both the intercept and slope, although the overall slope for the period from 1854 to 1892 remains negative. Starting in 1894, the trend line again shifts significantly in the positive direction. Although there is no associated shift in the level (intercept) of the time series, the slope for the third (1894 to 1930) time period is positive, indicating for the first time a trend toward less competition in congressional elections. The realigning period in the early 1930s resulted in a negative shift in the level of the time series (as manifested in a major, significant intercept shift), but the slope of the trend line for the fourth (1932 to 1964) period was not significantly different from the slope for the preceding period. Essentially, the similarity in slope coefficients for the third and fourth periods suggests that the latter process of change in election outcomes is merely a continuation of the former process of change, albeit at a different (lower) overall level. Finally, although the absolute *level* of competition is lower after 1965, the occurrence of a significant negative shift in the slope of the trend line reflects a post-1965 *trend* toward greater competition in congressional elections.

Table 1. Multiple Interrupted Time Series Results for Winners' Vote Proportions Over Time

Variable	B	Standard Error
Intercept	.6442*	.0082
COUNTER	–.0055*	.0009
DUMMY854	.0417*	.0102
COUNT854	.0038*	.0011
DUMMY894	.0113	.0097
COUNT894	.0066*	.0009
DUMMY932	–.0614*	.0101
COUNT932	–.0011	.0010
DUMMY966	.0225	.0137
COUNT966	–.0070*	.0024

$N = 79$ $R^2 = .8738$
*Significant at the .05 level.

Given these trends in vote proportions for northern congressional winners from 1824 to 1980, what can be said about the nature of change processes in congressional competition? First, it is clear that there has not been a monotonic increase in congressional winners' vote proportions since 1824. Much of the variation in patterns of congressional competition can be explained by each separate time period under consideration. It should be obvious that there is a slightly negative trend in winners' vote proportions during the 19th century and a positive trend during the 20th century, although even within these two centuries there are shifts in the parameter values of the time series which indicate patterns of change tied to realignment cycles.

Second, the different combinations of significant parameter shifts occurring at each intervention point in the time series suggest the occurrence of different types of change in congressional competition. For instance, the slope and intercept changes in the time series after 1854 indicate not only a change in the intensity of the underlying change process, but also the occurrence of an upward shift in the level of the process—perhaps in response to some combination of endogenous (process change) and exogenous (level change) factors. Likewise, the significant slope change for the period from 1894 to 1930 suggests the importance of the early 1890s as a source of major change in the process of determining congressional election outcomes. On the other hand, the change in congressional competition which occurred in 1932 can be seen as a short-term deviation from the post-1894 pattern. The resumption of the post-1894 trend (and the insignificant slope differential) following the 1932 intervention suggests the continuation of the underlying process that impacted upon congressional election outcomes between 1894 and 1930, albeit at a lower level. In other words, explanations of the post-1932 trend in congressional competition must look to the period from 1894 to 1930 to capture the nature of the underlying causal process.

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[8] We strongly agree with those who argue that it is erroneous to use significance tests on data consisting of an entire population. However, we recognize that others find that significance tests are appropriate for data such as ours. In deference to this position, and for illustrative purposes, the significance results from the multiple interrupted time series analysis have been reported throughout this article, although we do not feel compelled to base our inferences exclusively on these results.

[9] Although we are well aware of the problems associated with autocorrelated error terms in time series analysis (see, for instance, Ostrom, 1978), we have not presented the Durbin-Watson statistics and appropriate tests for serial correlation in our tables. Even though there is evidence of serial correlation in a few of our regression models, the results and substantive interpretation of our analysis are unchanged by the substitution into our tables of parameter values that are corrected for the effects of autocorrelation. Hence, all parameter values reported in our tables are OLS estimates.

Third, the MITS results demonstrate that questionable inferences about patterns of change in congressional competition from one time period to the next can result from undue aggregation of election results over time. A standard argument made in the literature on congressional elections is that, when compared with the period from 1946 to 1964, the post-1965 period exhibited a sharp increase in the trend toward less electoral competition. If one examines mean vote proportions for winning candidates during these consecutive time periods, this appears to be the case: the average winners' vote proportion for the pre-1965 period is .662, and is .686 for the post-1965 period—leaving a difference of .024. One might infer that there is an upward trend in winners' vote proportions, and a subsequent decline in congressional competition. Yet if one examines the MITS results and the plot of winners' vote proportions found in Figure 2, one can readily see a positive shift in the level (intercept) of the time series *and,* more important, a significant dampening of the previous upward trend which suggests a shift in the process of congressional election change over time. This is particularly important, because several scholars have argued that the post-1965 era can be best characterized by its trend toward decreasing competition in congressional elections. In fact, although our results are necessarily tentative owing to the few (eight) time points in the post-1965 period, they suggest a trend toward *increased* competition (i.e., decreased winners' vote proportions), or at least a discontinuity or leveling off of the long-term trend toward less congressional competition which began in 1894. Our empirical results lead us to posit that absolute levels of congressional competition are lower, but that *trends* in congressional competition are in the positive direction.

Explanations for Historical Trends in Congressional Competition

Of the four types of explanations in the literature used to explain the post-1965 trend toward greater congressional safety, the redistricting explanation offered by Tufte (1975) has been the most easily dispelled. Both Ferejohn (1977) and Fiorina (1977b) have presented solid evidence that calls into question the importance of redistricting to the post-1965 drop in congressional competition levels. The trends shown in Figure 2 offer additional long-term evidence that redistricting is not likely to be an adequate systematic explanation of major aggregate trends or shifts in patterns of congressional competition. The 1894-to-1930 and 1932-to-1946 trends toward electoral safety, as well as the 1932 shift point, simply do not correspond to any significant developments in redistricting.

Perhaps the dominant explanation of patterns of congressional competition found in the literature pertains to the incumbency advantage. This explanation attributes higher *levels* of congressional safety in the post-1965 period to specific electoral advantages that are unique to incumbent candidates. These advantages are seen as arising from either increased electoral resources (e.g., franking privilege, travel allowances, casework opportunities) or weak opponents, or else some combination of the two. Since electoral campaigns can be seen largely as a resource acquisition and distribution problem,[10] any factor that enhances the resource differential between competing candidates—whether based on incumbent resources or lack of challenger resources (i.e., weak challengers)—can be seen as contributing to the difference in vote proportions for incumbents and challengers.

From a long-term historical perspective, the importance of specific incumbent-unique resources in determining patterns of congressional competition seems suspect. For instance, Fiorina (1977a, b) suggests that it is no accident that congressional safety, the amount of casework, and the size of the federal bureaucracy all tended to increase in the mid-to-late 1960s. But when we

[10] By resource we refer to any commodity that, in the absence of equal counter resources, produces votes for a candidate. Some of the important resources for a candidate are party affiliation, individual appeal, image, political career, issue positions, name recognition, constituency contacts, and record of casework.

examine Figure 2, it is obvious that the dramatic decrease in congressional competition which occurred from 1896 to 1930 is simply not matched by major increases in federal expenditures. In fact, higher levels of federal expenditures in the mid-to-late 1930s correspond to higher levels of congressional competition than were seen in the 1920s. Thus, the higher levels of federal expenditures which are necessary to provide the basis for effective casework did not exist during the 1920s. A similar lack of correspondence between historically low levels of competition and other incumbent resources such as franking privilege and travel allowances casts doubt upon the historical applicability of specific incumbent resources as singular explanations of longitudinal variation in congressional competition. In terms of post-1965 developments in particular, there would seem to be a fundamental theoretical tension between the incumbency resource explanation and the results shown in Figure 2 and Table 1; that is, there does not appear to be any readily apparent reason why incumbent resources would dramatically lower the overall *level* of competition after 1965 while simultaneously reversing a *trend* toward less competition and initiating a trend toward greater competition.

Perhaps the best way to examine the impact of incumbency-unique advantages on patterns of congressional competition over time is to compare the electoral results (and subsequent contributions toward electoral competition and safety) of incumbent candidates with other types of candidates for whom the advantages of incumbency do not exist—namely, all nonincumbent candidates. Unfortunately, and as is already stated, such a comparison is generally inappropriate in that, although all incumbents can usually be considered viable candidates, many nonincumbents are not viable candidates. In our view, it is necessary to reduce the heterogeneity among nonincumbents which may distort comparisons used in testing alternative explanations of congressional election outcomes. Since electoral viability is difficult to define precisely, the easiest, and we feel best, way to undertake such comparisons is to restrict our analysis to winning candidates and compare winning nonincumbents with winning incumbents. Because congressional competition can be accurately specified on the basis of knowledge about the vote proportions of *winning* candidates, it is not necessary to consider nonwinning candidates in evaluating the impact of incumbency on patterns of congressional competition over time. Presumably, if incumbency-based factors account for cross-temporal patterns of congressional competition, the electoral results for incumbent winners should differ in a systematic fashion from those for nonincumbent winning candidates; otherwise, similarities between these two classes of candidates should be prevalent.

It is clearly the case that the advantages of incumbency are not likely to accrue equally to all incumbent candidates. Erikson (1971, 1976) has discussed the importance of the so-called "sophomore surge," which suggests differential impacts of incumbency factors for different classes of incumbent congressional candidates. Thus, in order to assess properly the impact of incumbency factors, it is useful to concentrate on differences and similarities in trends of vote proportions for nonincumbent winners, first-term incumbent winners, multiple-term incumbent winners, and all winners. If there is no advantage in the congressional election process which can be attributed to incumbency factors (whether to all or particular subsets of incumbents), one would expect *trends* in congressional election outcomes (i.e., as shown by slope coefficients for the time series) within each time period to be relatively uniform across subsets of congressional candidates. In other words, trends in vote proportions for each group of winners should be quite similar to the pattern exhibited by all winning candidates. On the other hand, if there is an incumbency advantage, one would expect trends (slopes) in vote proportions for incumbent winners (particular multiple-term incumbents) to surpass those for nonincumbent winners. Given this, declines in congressional competition could be attributed to processes that grant to incumbent winners some types of advantages over other winners.

MITS results for the plots of vote proportions over time for nonincumbent, first-term incumbent, and multi-term incumbent winners are shown in Table 2. What is striking about these results are the similarities in significant slope and intercept shifts for each subset of winners over time. In each case the pattern of change is similar: a negative slope in the pre-1854 period, a positive (and statistically significant) shift in both the level (intercept) and trend (slope) of the time series from 1854 to 1892, a significant upward shift in the trend (but not the level) after 1894, and a sharp decrease in the level (intercept) in the post-1932 period accompanied by a continuation of the post-1894 trend through 1964. After 1930, there are two cases in which there are significant deviations from the uniformity of these patterns. First, there is a significant negative slope shift in the trend of vote proportions for nonincumbent winners after 1932 which suggests a slight trend toward greater competition in those elections involving a nonincumbent winner, whereas there is no significant interruption in the same trend for all winners (see Table 1) and first-term and multiple-term incumbent winners. Second, all winners and first-term incumbent winners under-

Table 2. Multiple Interrupted Time Series Results for Vote Proportions of Nonincumbent, First-term Incumbent, and Multiple-term Incumbent Winners Over Time

	Nonincumbent		First-term Incumbent		Multi-term Incumbent	
Variable	*B*	*SE*	*B*	*SE*	*B*	*SE*
Intercept	.5964*	.0077	.7058*	.0162	.8458*	.0205
COUNTER	–.0033*	.0009	–.0079*	.0018	–.0189*	.0023
DUMMY854	.0031*	.0096	.0489*	.0201	.0693*	.0255
COUNT854	.0019**	.0010	.0051*	.0021	.0170*	.0027
DUMMY894	.0107	.0092	.0250	.0191	.0168	.0243
COUNT894	.0033*	.0008	.0058*	.0017	.0066*	.0022
DUMMY932	–.0292*	.0096	–.0458*	.0200	–.0507*	.0254
COUNT932	–.0018**	.0009	.0025	.0019	–.0021	.0024
DUMMY966	.0149	.0129	.0131	.0270	.0239	.0342
COUNT966	.0019	.0023	–.0134*	.0048	–.0041	.0061

N = 79 $R^2 = .4592$ $R^2 = .6185$ $R^2 = .6785$

*Significant at .05 level.
**Significant at .10 level.

went a significant negative shift in the trend (slope) of vote proportions over time after 1965, whereas nonincumbent winners experienced a small (nonsignificant) increase and multiterm incumbent winners a small (nonsignificant) decrease in the trend of vote proportions. Essentially, the overall trend in the post-1965 era toward increased congressional competition can be attributed primarily to the significant increase of the trend in congressional competition among first-term incumbent winners, and can perhaps be secondarily attributed to the weak increased competitive trend among multiple-term incumbent winners.

We can also examine the differences in intensity of changes in the longitudinal trends of vote proportions for each class of winning candidates in order to assess the differing contributions made in determining overall trends in competition. During the first (1824 to 1852) period of these three time series, it appears that by virtue of their large negative slope coefficients for that time period, multiple-term incumbents made a disproportionately large contribution to the competitive trend in vote proportions; this is not surprising, given the initial high levels of electoral safety for multiterm incumbents at the very beginning of the time series. In the second (1854 to 1892) time period, there is an overall positive shift in the trend in vote proportions which is the result of a positive shift for all three classes of winners, although in each subset of candidates the overall slope remains negative. However, it appears that incumbency status is an important factor in this process of trend change, for the slope shift is greatest for multiterm incumbent winners, second for first-term incumbent winners, and the least for nonincumbent winners.[11]

After 1894 there is a trend toward increased winners' vote proportions (and hence, decreased competition), with multiterm incumbents again providing the greatest contribution in this trend. The rate of improvement in vote proportions for multiterm incumbent winners during this period exceeds that of non-incumbent and first-term incumbent winners, suggesting a disproportionate impact on the overall trend in competition.[12]

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[11] During the first two time periods (1824 to 1894), nonincumbent winners were the largest subset of all winners. As such, they consistently made the largest overall contribution to the slope coefficient for all winners. This does not negate the importance of incumbency because the contribution of incumbents to the actual slope and intercept changes remained disproportionate to their numbers.

[12] Incumbents also provided a disproportionate share to the trend toward increased winners' proportions over the period of 1894 to 1964 because over this period the proportion of all winners who were incumbents tended to increase dramatically as shown in the previous work of Polsby (1969) and Price (1975). However, it is equally clear that the increase in the vote proportions of winning congressional candidates cannot be attributed solely to the increased proportion of winners who are incumbents. If one examines the trend lines—shown in Table 2—for first-term and multiple-term incumbent winners, one finds that the slopes for the post-1894 period are all positive. Since both trends are independent of the number of incumbents winning, and since the trend for first-term incumbents is independent of the number of multiple-term incumbents, factors other than the mere number of incumbents must be affecting the

After 1932 the results became a bit more ambiguous. For all winning candidates there is a slightly negative, nonsignificant shift in the trend of vote proportions. However, the trend behavior for the three classes of winners is not uniform. Nonincumbent winners exhibit a significant negative slope change, indicating a slight trend toward greater competition. On the other hand, the slope shifts for both incumbent classes of winners is nonsignificant, although first-term incumbent winners are characterized by a slight positive trend shift and multiterm incumbent winners by a slightly negative trend shift. In other words, the rate of increase in patterns of vote proportions (i.e., congressional safety) for first-term incumbent winners is greater than that for nonincumbent and multiterm incumbent winners, although the significance levels make such a conclusion highly tentative.[13]

Perhaps the most surprising set of results occurs during the final period of the time series. The literature on congressional elections posits that the post-1965 period has been characterized by an increase in the electoral advantage of incumbent candidates. If one only focuses on the absolute *level* of competition, this would appear to be the case. However, one would also expect the increasing importance of incumbency to be reflected in the slope coefficients by the exhibition of progressively higher shifts as incumbency status increases. Yet this does not seem to be the case. In fact, it appears that nonincumbent winners are the only group to enjoy a positive shift in the trend of vote proportions, whereas both classes of incumbent winners show a decline in their trend as compared to the previous period. In addition, nonincumbent winners are the only candidates who exhibit an overall positive slope (i.e., trend toward increased electoral safety) in the post-1965 period. Both first-term and multi-term incumbents exhibit trends toward greater competition over time during the most recent era, although the actual level (intercept) of competition is lower after 1965 than before. In any event, such findings call into serious question the validity of previous work on congressional elections which posits an increasing trend toward incumbency advantage in the post-1965 era.

A more proper specification of the incumbency advantage can be established by focusing directly on the difference between the electoral performance of incumbent and nonincumbent winning candidates. If viable incumbent candidates hold electoral advantages over viable nonincumbent candidates, one might expect to see the vote proportions for the former to be greater than the vote proportions for the latter; hence, the difference in vote proportions at time *t* between viable (winning) incumbents and nonincumbents should represent adequately the concept of incumbency advantage. In addition, the plot of these differences over time should give us an accurate portrayal of longitudinal trends in incumbency advantage. In order to specify more properly patterns of incumbenty advantage over time, difference scores between all incumbent winners, first-term incumbent winners, and multiterm incumbent winners, on one hand, and nonincumbent winners, on the other, were calculated. These scores represent the advantage that all incumbent winners, first-term incumbent winners, and multiterm incumbent winners, respectively, have over viable (winning) nonincumbents.

The MITS results for the plots of these three sets of difference scores over time are shown in Table 3. In addition, Figure 3 shows the difference scores for all incumbent winners and nonincumbent winners from 1824 to 1980. These results provide a striking contrast to that which would be expected, given the current state of the literature. The first and most important conclusion that can be deduced from Figure 3 is that there is *no* gain for incumbent over nonincumbent winners in the post-1965 era. The post-1965 decrease in the *level* of congressional competition is, therefore, more properly seen as an increase in winners' advantage and not solely an increase in incumbency advantage. The immediate importance of this finding is that it calls into question explanations that are based upon the sole importance of casework and other incumbent-exclusive resources. Such explanations simply cannot explain why nonincumbent winners are also doing better after 1965.

Figure 3 and the results shown in Table 3 indicate that, from 1894 to the present, incumbent winners generally increased their average electoral

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trends toward less competition in elections involving incumbents and thus the general trend toward less competition in all congressional elections.

[13] Here again, the issue of applying significance tests to population trends arises. If we base our inferences solely on the statistical significance of intervention shifts (i.e., if we treat the counter coefficients as *estimates* of parameter shifts), we cannot draw any conclusions about parameter shifts for trends in vote proportions for both classes of incumbent winners. In this case, we must interpret the insignificance of the results as suggesting that *no* shift had occurred (i.e., that the slope before intervention is equal to the slope after intervention). On the other hand, we can infer the existence of trend changes if we treat the slope and intercept coefficients as actual population parameters. Throughout this article we have reported significance tests of regression results in deference to those who argue for their use in analyses such as ours. However, we do not feel constrained to be bound by these significance results in interpreting our data and findings.

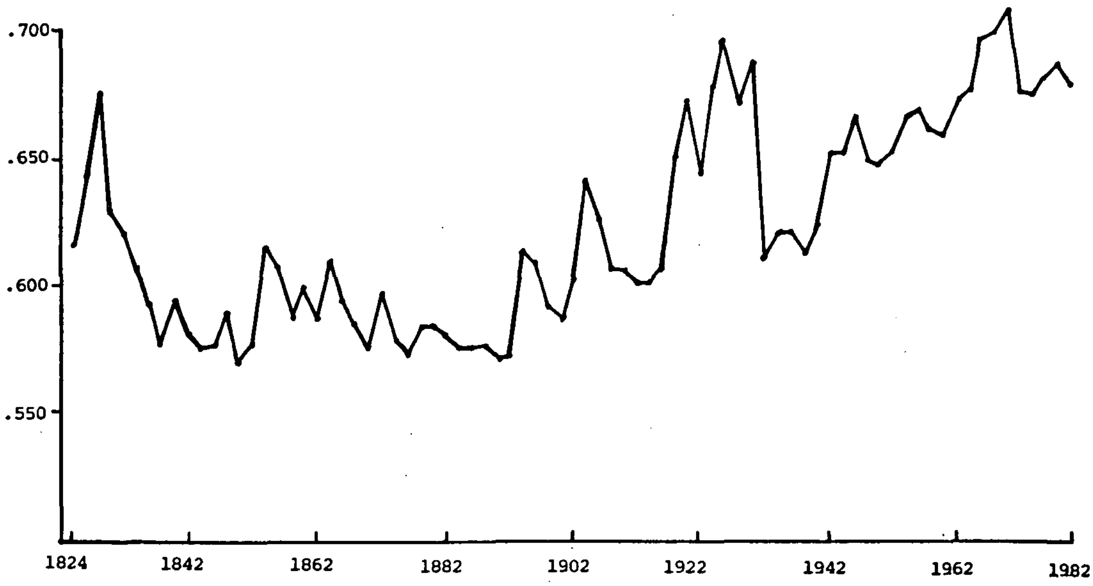
Table 3. Multiple Interrupted Time Series Results for Difference Scores Over Time: All Incumbent Winners, First-term Incumbent Winners, and Multiple-term Incumbent Winners

	All		First-Term		Multi-Term	
Variable	*B*	*SE*	*B*	*SE*	*B*	*SE*
Intercept	.1460*	.0115	.1093*	.0171	.2493*	.0206
COUNTER	–.0071*	.0013	–.0046*	.0019	–.0156*	.0023
DUMMY854	.0175	.0143	.0178	.0213	.0382	.0256
COUNT854	.0061*	.0015	.0032	.0022	.0151*	.0027
DUMMY894	.0059	.0136	.0142	.0202	.0061	.0243
COUNT894	.0039*	.0012	.0026	.0018	.0033	.0022
DUMMY932	–.0256**	.0142	–.0166	.0211	–.0215	.0254
COUNT932	.0005	.0014	.0043*	.0020	–.0003	.0025
DUMMY966	.0057	.0192	–.0018	.0285	.0089	.0343
COUNT966	–.0081*	.0034	–.0153*	.0051	–.0060	.0061
$N = 79$	$R^2 = .6974$		$R^2 = .4952$		$R^2 = .5911$	

*Significant at .05 level.
**Significant at .10 level.

advantage over nonincumbent winners.[14] This trend toward a greater disparity between incumbent and nonincumbent winners seems to have levelled off, and perhaps has been reduced, since the mid-1960s. Where the average difference was approximately 3 to 5% in 1896 to 1898, the difference peaked at approximately 16.5% in 1966. After 1965 a definite negative trend in the differences between incumbent and nonincumbent winners is evident; an examination of the slope coefficients for the two classes of incumbents suggests that although there was a clear (but statistically insignificant) erosion in the incumbency advantage for both first-term and multiterm incumbents after 1965, the former incumbent group contributed the greatest amount toward the decrease in the incumbency advantage. Nonetheless, one must conclude that it is during the era from 1894 to 1964 that there is an almost

Figure 3. Difference Scores for All Incumbent and Non-Incumbent Winners, 1824-1980



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[14] We do not concentrate our efforts on difference scores for the period of 1824 to 1892, although it should be noted that there is a negative trend in the difference scores from 1828 to 1840.

incumbent-exclusive growth in electoral margins.[15] Any incumbent advantage that remains after 1965 appears to be the result of this earlier trend, but also seems to have eroded somewhat since 1965—primarily (but not exclusively) as a result of a declining advantage among first-term incumbent winners.

Thus far, it would seem that redistricting and specific incumbency-unique resources do not provide adequate singular explanations of either the pre- or post-1965 developments in congressional competition. We are left with two explanations: weak opponents and changes in the electorate. Although our data are generally insufficient to provide a direct test between these two alternative explanations, the data in Figure 3 suggest a theoretical problem in the weak opponents argument. Why should the benefits of weak opponents accrue primarily to incumbent winners before 1965 and to nonincumbent winners and (to a lesser extent) incumbent winners after 1965? Unless reasons can be developed to explain why opponents to incumbent winners became weaker from 1894 to 1980 whereas opponents to nonincumbents became weaker only after 1965, the weak opponents argument must be considered suspect.

How, then, can we explain patterns of congressional competition over time? In our view, the only explanation that comes close to fitting the data that we have presented here is the changing electorate argument similar to that found in the writings of Burnham (1970, 1974, 1975a, b). Central to Burnham's explanation is the concept of party disaggregation. As voters' attachment to the major parties decreases—manifested in increasing numbers of independents and more split-ticket voting—the parties' role as action intermediaries in electoral choice is diminished. As electoral disaggregation continues, party cues are replaced by other types of cues in the voter's decisional calculus.

According to Burnham, one result of party disaggregation is a lower level of competition in aggregate electoral outcomes. Burnham notes the trend toward lower congressional competition beginning in the mid-1890s and suggests a linkage between this trend and the trend toward greater party disaggregation beginning around the same time. Our data seem to be compatible with this explanation. As was shown in Figure 2, winners' vote proportions increased dramatically between 1894 and the 1920s, which corresponds exactly with Burnham's first period or movement of party disaggregation and the resulting shift to a mercantilist campaign style.[16] Burnham sees 1932 as a temporary respite in the trend toward party disaggregation with partisanship being reinvigorated as a result of the polarity induced by the Great Depression. Here again the trend shown in Figure 2 corresponds nicely to Burnham's argument. Finally, once the shock to the electoral system brought about by the Depression died down following the election of 1932, there was a return to the trend toward greater party disaggregation and, subsequently, toward lower levels of congressional competition.

The fact that major changes in the trend line in Figure 2 correspond to what are widely recognized as realigning periods lends strong support to the changing electoral explanation of change in congressional competition. But what about post-1965 patterns exhibited in Figure 3? One might argue that this trend appears to contradict the arguments posited by Burnham. We disagree. First, the apparent levelling off of the difference in the vote margin of incumbent winners vis-a-vis nonincumbent winners after 1965 corresponds to widely recognized changes in the electorate. The mid-1960s may not be considered a realigning period, but there were major changes in the number of Independent-identifying voters, increases in the amount of split ticket voting, and increases in what is generally referred to as the volatility of the electorate. Second, as Burnham points out, the impact of a changing electorate must be viewed in terms of the ability of politicos to respond to the changes. Taking this perspective, what is important about the 1960s is that it is the beginning of an era of new campaign technologies—opinion polling, well-financed television campaigns and other media events, and mass mailings—which, unlike casework and the pork barrel, are available to incumbent and nonincumbent candidates alike. Thus, it is possible that the nature of new campaign technologies allows nonincumbent congressional candidates, as well as incumbent candidates, to take greater advantage of a changing electorate.

We are not suggesting that Burnham's (or anyone else's) changing electorate explanation of longitudinal patterns of congressional competition has been upheld by our analysis. On the contrary, we suggest the need for additional research into the linkage between party disaggregation and congressional competition. However, we do contend that the changing electorate class of explanations is the only one that is compatible with our data and analysis. The other explanations offered in the literature—reapportionment, incumbency advantages, and weak opponents—do not address changes in congressional competition over the entire 1824 to 1980 time series adequately enough to convince us of their empirical validity.

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[15] Of course, since throughout most of the 20th century the majority of winners were incumbents, they make a disproportionately large contribution to the total upward trend for winners. Although we do not present the data, incumbent losers lose at about the same rate after 1965 as they did in the period from 1946 to 1964.

[16] Burnham borrows the term "mercantilist" and "militarist" campaign styles from the work of Jenson (1968).

In this context it is particularly important to recognize the need for additional research on the relationship between party disaggregation and partisan realignments. Although both of these concepts emphasize the importance of changes in the electorate, each focuses on a related but different type of change. Although the partisan realignment literature is concerned primarily with changes in the electoral support for particular parties, the emphasis in the literature on party aggregation-disaggregation is on change in the commitment of the electorate to parties in general. It is likely that these two types of change are related in an ongoing, interactive, and dynamic manner. For instance, as the electorate changes in its commitment to political parties in general, such a change is likely to affect the nature of subsequent realignments. Likewise, a particular realignment can affect the future commitment of the electorate to political parties in general.

The data that we have presented in this article indicate trends toward greater competition before 1894 and trends toward less competition after 1894. Assuming an inverse relationship between party disaggregation and electoral (congressional) competition and an expected interaction between party disaggregation and partisan realignments, one would expect to see differences in the nature of the realignments of the 1850s, 1890s, and 1930s as a result of this interaction. Although it is beyond the scope of this study to undertake such an analysis, the recent work of Clubb, Flanigan, and Zingale (1980) suggests that this is indeed the case. In their analysis these authors indicate that the realignments of the 1850s, 1890s, and 1930s each had a number of unique characteristics. In particular, Clubb, Flanigan, and Zingale indicate that the realignment of the 1930s began to deteriorate most rapidly, whereas the realignment of the 1850s was the slowest to deteriorate. Given that the realignment of the 1930s took place in a period of growing party disaggregation, or lack of commitment to parties in general, it is not particularly surprising to find that, after a temporary reinvigoration of parties as a result of the Great Depression era, the coalition based upon this reinvigoration began to break down quite rapidly. The realignment of the 1850s, on the other hand, can be seen as having deteriorated at a much slower rate because it occurred during a time of growing party aggregation (i.e., growing commitment to political parties in general). What appears most unique about the 1890s is that during this time the nation not only underwent a partisan realignment but also a fundamental change in the process of party aggregation-disaggregation.

Implications and Conclusions

What can be said, then, about patterns of congressional competition during the period from 1824 to 1980? Given the preceding analysis, we can draw several important conclusions. First, trends in competition for congressional elections are not as easily specified as one might believe; this is important, because a mis-specification of cross-temporal patterns of congressional competition may result in invalid inferences about the causes of such patterns. For instance, in our view it is improper to infer an increasing trend in congressional safety on the basis of aggregation of electoral outcomes (i.e., means) from one period to the next. Such a strategy may mask actual trends that can be uncovered only through an examination of trends in the entire time series.

Second, it is clear that there has been a long-term trend toward congressional safety since 1932, and perhaps even since 1894. This trend calls into serious question research that suggests the unique nature of post-1965 levels of congressional competition and that tries to explain these levels on the basis of post-1965 developments and incumbent-unique resources. In fact, one of our tentative findings is that there is actually a negative trend in electoral safety in the post-1965 period, although a level shift occurring around 1965 has resulted in a higher average level of safety from 1966 to 1980.

Third, the incumbency-based explanations of the post-1965 levels of congressional safety which dominate the literature must be viewed as suspect, given the trend in winners' vote proportions and differences in incumbent winners and nonincumbent winners vote proportions over time. Post-1896 trends require us to look for explanations that are applicable to a long time series. Incumbent winners have always done better than nonincumbent winners, but the margin of difference increased from 1896 to 1966 and has stabilized, or begun to decrease, thereafter. The growth in incumbency advantage would appear to be a pre-1965 development. Because incumbent winners are not doing any better vis-a-vis nonincumbent winners after 1965, it becomes necessary to speak of a recently developed "winners' advantage" and not an "incumbency advantage."

Perhaps the only set of explanations that conform with our data are those that explain changes in the level of congressional competition on the basis of changes in the electorate. The conformity of changes in congressional competition and known changes in the electorate during realigning periods seems to suggest the possible applicability of these explanations. Although the nature of our data does not allow us to examine the differences among various changing electorate arguments, the findings of this article should provide a useful basis for future research on congressional competition.

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Free Association and the Theory of Proportional Representation

ROBERT SUGDEN
University of Newcastle upon Tyne

Several of the earliest advocates of proportional representation upheld the "principle of free association": that electors should be free to choose the constituencies to which they belong. This article analyzes this principle in game-theoretic terms and investigates how far the free association "game" can be simulated through proportional representation. It is assumed that each voter's prime concern is that his most-preferred candidate should be elected. If preferences are single peaked, the outcome of an election based on free association can be predicted; the same outcome would result from a modified form of the single transferable vote.

Duncan Black, in his pioneering work *The Theory of Committees and Elections* (1958), discussed proportional representation with some care and with particular reference to the single transferable vote (STV) system. A scheme of proportional representation, he wrote, could not be called satisfactory unless it was "a mathematical scheme, stating a unitary principle, and not merely an arithmetic rule of thumb" (p. 76). The STV system, he concluded, was not a mathematical scheme in this sense, but merely "a practical directive for counting votes and electing candidates." However, he conceded that he was unable to supply a suitable unitary principle; thus to the "essential question" of whether STV was a just system, "we cannot (he wrote) give any proper answer . . . since we ourselves have been unable to arrive at a scheme which we regard as satisfactory and which might afford some criterion for the satisfactoriness of other methods" (p. 80). Although most of Black's insights have since been explored and developed with great rigor by social choice theorists, this particular problem has not, to my knowledge, yet been satisfactorily resolved. The purpose of this article is to offer a solution. I shall present a unitary principle against which practical schemes of proportional representation might be evaluated; I shall show that this was the principle to which the earliest proponents of STV appealed; and then I shall investigate how far this principle really does provide a justification for STV.

The Principle of Free Association

Supporters of the idea of proportional representation say that a representative assembly should represent all interests and opinions in proportion to their numerical strength in society. Two arguments are commonly offered in support of this principle. The first might be summarized as *the right to be heard.* Democracy, say the proponents of proportional representation, means government by the whole people and not just a majority of them. When it comes to a final decision, it may be democratic that the will of the majority should prevail; but in the process of deliberation and debate, all opinions should, as far as possible, be heard. Thus in designing a system of representation the ideal to be aimed for is an assembly in which every citizen has someone who will speak for him. Carl Andrae, one of the inventors of STV, put this idea clearly:

> In final decisions the majority should indeed rule and its will be respected, but so long as we are dealing with deliberation . . . so long should we not shut the mouth of minorities; so long should we give them a chance to come forth in proportion to their strength and express their differing opinions quoted by his son, Andrae, 1926, pp. 11-12).

In a similar vein, John Stuart Mill wrote in support of STV that Parliament should be

> an arena . . . where every person in the country may count on finding someone who speaks his

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> mind, as well or better than he could speak it himself (1861, ch. 5).

On this argument it is not enough that each citizen belongs to a constituency that has a representative in the assembly, because if the representative does not speak the citizen's mind, the representation is more nominal than real.

The second (and closely related) argument for proportional representation is that there should be *no double counting of majorities.* Even if democracy is construed to mean no more than that the will of the majority should prevail, it may be undemocratic to exclude minorities from a representative assembly. If a final decision requires only the approval of a majority in the assembly, and if the whole assembly represents only a majority of citizens, the final decision may be approved by people who represent little more than 25% of the citizens (cf. Buchanan & Tullock, 1962, p. 242; Mill, 1861, ch. 7). Representation should be designed, it is argued, so that any majority of representatives are in a real rather than nominal sense the representatives of a majority of the citizens.

Thus, one might argue, it is not enough to divide the citizens into equally sized constituencies: the constituencies must be formed so that people with similar opinions or interests are grouped together. Some procedure is needed to group people together in this way. But what could this procedure be? Any procedure that does not rely on voters choosing their own fellow constituents is fundamentally arbitrary, since it must rely on some a priori ideas about the sort of diversity that is to be represented. It is not surprising, therefore, that the early theorists of proportional representation took as their ideal what I shall call the *principle of free association:* citizens should be free to choose for themselves which constituencies they belong to.

Andrae defended his STV system as an approximation to an ideal of elections based on free association. Suppose, he wrote, that 65 representatives are to be chosen by a society of 13,000 voters:

> then it is clear that 200 voters must be said to have a natural right of being represented by one representative. *This is very simple and natural; and according to my conviction it is also the right idea, the aim towards which all electoral laws should progress.* . . . I am inclined to believe that there would be no danger whatever in making the elections entirely unrestricted on the basis I have indicated [i.e., allowing any voluntary association of the appropriate number of voters to nominate its own representative] (quoted by his son, Andrae, 1926, pp. 12-13; emphasis in original).

Thomas Hare, who is usually credited with the invention of STV, presented a very similar argument:

> The ordinary man is not precluded from choosing his friends or associates beyond the boundaries of his own borough, and there does not seem to be any sound reason why he should not be allowed, with a like freedom, to seek elsewhere his fellow-constituents. If the legal *obstacles in the way of this exercise of individual* volition were removed, and the elector were enabled to add his votes to the votes of any other of his countrymen, agreeing with him in sympathy and opinion, and sufficient to form a constituency, the question as to minorities [being unrepresented] would cease (1873, p. 21).

Mill supported Hare's proposal for electing Parliament by STV, saying that:

> Real equality of representation is not obtained unless any set of electors amounting to the average number in a constituency, wherever in the country they happen to reside, have the power of combining together to return a representative (1861, ch. 7).

Andrae, Hare, and Mill are all arguing that any group of citizens, provided it is large enough to qualify as a constituency and provided that all its members can agree on a nomination, should be allowed to have the representative of its choice. (Each citizen, of course, should be permitted to join no more than one such group.) But how large must a group be to qualify as a constituency? All three writers seem to imply that the qualifying size of group—the *quota*—should be equal to the ratio between the number of electors and the number of representatives. However, it is not necessary to set the quota so high. If m representatives are to be chosen and if n citizens take part in choosing them, the quota may be set at any number greater than $n/(m + 1)$ without there being any possibility of more than m candidates qualifying for election. Thus there is a strong case for setting the quota at the smallest integer greater than $n/(m + 1)$: this, in the context of STV, is the *Droop quota.* If this case is accepted, the principle of free association may be formulated more precisely as follows: any group of more than $n/(m + 1)$ citizens, all of whom agree on a nomination, should be allowed to have the representative of its choice.

One can conceive of an electoral system in which this principle would be the sole rule, voters being left free to bargain among themselves as they saw fit. Bargaining would be essential if the principle were to work. For example, suppose that a minority group is not large enough to qualify for a representative of its own, but wants its interests to be represented in the assembly. Its best strategy might be to combine with some other minority group to nominate an agreed candidate, but this might be possible only if both groups were prepared to compromise. This kind of bargaining must be distinguished from the kind that involves side payments—for example, a deal by which a voter agrees to nominate a particular candidate in return for a cash payment. Most people, I suggest, would regard this latter kind of bargaining as improper. I shall therefore assume that an electoral system based on the principle of free association would incorporate a rule forbidding all side payments. I shall call the system consisting of these two rules—the principle of free association and the prohibition of side payments—*election by free association,* or EFA for short. A historical example of an election of essentially this kind has been described by Enid Lakeman (1974, p. 114) who is one of the most forceful modern advocates of STV, and interestingly, she claims that this is "an admirable example of the use of the STV."

Election by free association can be regarded as a game of strategy in which voters bargain and coalesce with one another in an attempt to influence the composition of the assembly. I shall use some of the concepts of game theory to analyze EFA and to predict the outcomes if that system were put into practice. I shall then investigate how far STV can be interpreted as an approximation to, or a simulation of, EFA.

Election by Free Association: A Game-Theoretic Analysis

First let me present some notation and assumptions.

> *Assumption 1. Candidates, seats and voters.* There is a set S of candidates; the number of candidates is M. There are m seats to be filled, where $M > m > 0$. There are n voters, where $n > m$. The quota, q, is the smallest integer greater than $n/(m + 1)$.

> *Assumption 2. The number of voters.* The number of voters is such that $(n + 1)/(m + 1)$ is an integer; hence $q = (n + 1)/(m + 1)$.

Assumption 2 deserves some explanation. First, this assumption is not without precedent. In the special case where $m = 1$ (the case of an election in which there is only one seat to fill), Assumption 2 entails that the number of voters is odd. This particular assumption is often used in social choice theory, for reasons of convenience. It turns out that a generalization of the "odd number" assumption has a similar value in the analysis of EFA. The reason, essentially, is that under the rules of EFA, the smallest group that can secure the election of one candidate is a group of q voters. Hence the smallest group that can secure the election of m candidates—that is, that can dictate how every seat is filled—is a group of mq voters. Thus any group of at least $n - mq + 1$ voters has some negative or blocking power: it is impossible for the voters outside such a group to fill all m seats without some support from within the group. Assumption 2 entails that $n - mq + 1 = q$. Thus the smallest group with blocking power is the same size as the smallest group with the power to elect a candidate.

This argument may help to explain why Assumption 2 is *convenient,* but it does not make it any more *realistic.* Taken literally, this assumption is undoubtedly restrictive. In the simplest case, where $m = 1$, it requires that the number of voters should be odd, or equivalently, that $n + 1$ should be a multiple of 2. Taking any large electorate at random, it would seem that there is only a 50% chance that the assumption will hold. If $m = 2$, the assumption requires that $n + 1$ should be a multiple of 3, which will be true only 33% of the time. And so on. Nonetheless, the assumption would be defensible if it could be shown that what is *exactly* true when the assumption holds is *approximately* true when it does not. In the Appendix, I show that this is in fact the case.

In the theoretical sense, EFA is a game in which the voters are the players. Each voter must choose one of $M + 1$ possible strategies: he may nominate one of the M candidates, or he may nominate no one. The list of the strategies chosen by the n voters is, in the language of game theory, the *strategy n-tuple.* The rules of EFA prescribe an outcome for every possible strategy n-tuple; the outcome is the set of candidates who are elected and is made up of all those candidates who have been nominated by at least q voters. It is impossible for more than m candidates to be elected, but it is possible that fewer than m candidates—or even none at all—are elected. To analyze EFA in game theoretic terms, voters' preferences must be defined over all possible outcomes.

> *Assumption 3. Preferences over slates.* Any subset of S that contains no more than m candidates is a slate. Each voter has a strict preference ordering on the set of all logically possible slates.

That voters have preference orderings over slates—that is, over the possible outcomes of the election—should be uncontroversial. The additional assumption that these preferences are strict—that a voter is never indifferent between slates—is a convenient simplification.

EFA is a game of bargaining and coalition forming, a cooperative game. For such games the most natural solution concept, and the one most widely used by game theorists, is the *core.* In the context of EFA, this concept may be defined as follows:

> *Blocking.* A slate A is blocked if and only if there exists a set of voters V, and a particular strategy for each voter in V, such that if these strategies are followed then, irrespective of what the other voters do, the outcome will be a slate that every member of V prefers to A.
>
> *Core.* The core is the set of all unblocked slates.

Now consider the special case in which $m = 1$, in which there is only one seat to be filled. By Assumption 2 the number of voters is odd, and q is the smallest integer greater than $n/2$, that is, a bare majority of the voters. Under the rules of EFA, any group of q voters can guarantee the election of any one candidate; and since there is only one seat to be filled, this amounts to determining the winning slate. A group containing fewer than q voters cannot guarantee either the election or the nonelection of any candidate. Thus in this case a slate (or candidate: the two are equivalent when $m = 1$) is blocked if and only if there exists some other slate that is preferred to the former by a majority of the voters. So the core (if it is not empty) contains just one one-candidate slate $\{x\}$ where x is the candidate (if there is one) who can beat every other candidate in pairwise contests by majority vote. That such a candidate ought to be chosen is the well-known Condorcet criterion (Black, 1958).

When $m > 1$, however, a slate that satisfies the Condorcet criterion is not necessarily in the core of the EFA game. Here is a simple example. Suppose that $m = 2$ and $n = 8$; thus $q = 3$. There are three candidates, x, y, z, competing for the two seats. Five voters agree that the best of all possible slates is $\{x,y\}$, followed by $\{x,z\}$ and then $\{x\}$. (Notice that these are the three slates that include x.) The other three voters believe that the best slate is $\{y,z\}$, followed by $\{x,z\}$ and then $\{z\}$. (Notice that these are the three slates that include z.) Clearly, the Condorcet criterion would choose $\{x,y\}$; since it is the most preferred slate for a majority of the voters, it must be able to beat any other slate in a pairwise contest by majority vote. Nonetheless, given the rules of the EFA game, $\{x,y\}$ can be blocked. The three voters in the minority have the power to elect candidate z, and thus to ensure that the outcome is either $\{x,z\}$, $\{y,z\}$, or $\{z\}$. Since these voters would prefer any of these outcomes to $\{x,y\}$, $\{x,y\}$ is blocked. The only slate that cannot be blocked is $\{x,z\}$. (The five voters in the majority can block any slate that excludes x, and the three voters in the minority can block any slate that excludes z. Neither group, acting on its own, can guarantee an outcome that its members prefer to $\{x,z\}$, and since there is no slate that both groups prefer to $\{x,z\}$, this slate cannot be blocked by the two groups acting in concert.)

This example helps to show how, when more than one seat has to be filled, the outcome of EFA may be fairer than the outcome of using the Condorcet criterion. The Condorcet criterion would allow a bare majority of voters to determine how every seat should be filled: if five of the eight voters want x and y to be elected, x and y are elected. In contrast, EFA gives some weight to the preferences of minorities, allowing the three voters who want z to be elected to have their way. EFA respects the minority's "right to be heard" in a way that the Condorcet criterion does not.

As the example may suggest, the EFA game is rather difficult to analyze when $m > 1$. To make the problem tractable, I shall make the following assumption about voters' preferences.

> *Assumption 4. Lexicographic preferences.* Each voter has a strict preference ordering on S, the set of candidates. A voter's preferences between slates are derived lexicographically from his preferences between candidates; that is, a voter prefers slate A_1 to slate A_2 if and only if *either* (i) after ignoring all those candidates who are common to A_1 and A_2, the most preferred candidate in A_1 is preferred to the most preferred candidate in A_2 *or* (ii) A_2 is a proper subset of A_1.

This is a strong assumption that requires some defense.

My ultimate purpose is to evaluate schemes of proportional representation in general and STV in particular. Practical schemes of proportional representation generally do not ask the voter to record an ordering of all possible slates: if m and M were at all large, this would be far too time-consuming a task. (For example, if $m = 3$ and $M = 10$, here are 176 possible slates.) Under STV each voter records an ordering of *candidates.* But when one is evaluating STV, it is natural to ask how the outcomes of STV relate to voters' preferences, and the outcomes are slates, not candidates. Thus there is no escaping the necessity for an assumption about how preferences over candidates relate to preferences over slates.

To say that some simplifying assumption is unavoidable is not of course enough to justify a particular assumption. Assumption 4 corresponds with a particular view of the purpose of representation. It says that each voter's prime concern is that the candidate he most prefers should be elected. This makes sense if one holds that the principal job of a representative is to act as a spokesman or advocate for the people he represents: his job is to ensure that the opinions of his constituents are presented in public. And this in turn fits naturally with the idea that a representative assembly is primarily a forum for debate—an idea that is implied very strongly in some of the passages I quoted from Andrae and Mill.

It is perfectly possible to take a different view of the purpose of representation. One might consider a representative assembly not as a forum for debate but as an institution for decision making; one might say that the representative's main job is not to *speak* on behalf of his constituents but to *vote* on behalf of them. On this view little is served by having a representative who is consistently outvoted in the assembly; presumably voters would be more concerned with the composition of the assembly as a whole than with the election of any particular candidate.

So Assumption 4 is just one of perhaps many possible starting points for an analysis of proportional representation. However, it is a starting point that seems to be in tune with the arguments presented by the earliest advocates of proportional representation. I shall show that this particular assumption leads us in the direction of a justification for STV (although it does not take us all the way). It may be that alternative assumptions would lead to justifications of other schemes of proportional representation, such as the list system. Even so, it is useful to know the grounds on which STV might be defended.

Given Assumption 4, the concept of blocking can be formulated more precisely. Let A be any slate, and suppose that it is blocked by a particular coalition of voters. Then this coalition must have the power to guarantee the election of certain candidates (whom I shall call the *nominees* of the coalition), and every member of the coalition must prefer any slate containing those nominees to the original slate A. The former condition will be satisfied if and only if the coalition contains at least q voters for every nominee. Now consider the latter condition. Call a nominee a *repeat nominee* if he is a member of A and a *new nominee* if he is not. If a voter prefers at least one of the new nominees to every candidate in A who is not a nominee, then (because of Assumption 4) he must prefer any slate containing the nominees to A. Conversely, suppose that he prefers one of the un-nominated candidates in A, say x, to all the new nominees. Let A' be any slate that contains all the nominees, excludes x, and contains no candidate who is not either a nominee or a member of A. Then the voter in question must prefer A to A', and so he does *not* prefer every slate containing the nominees to A. These conclusions may be summarized as follows.

> *Necessary and sufficient conditions for blocking.* A slate A is blocked if and only if there exist (i) p repeat nominees (where $p \geqslant 0$) drawn from A, and (ii) r nominees (where $r > 0$) drawn from $S - A$, and (iii) a coalition of $(p + r)q$ voters such that each member of the coalition prefers at least one of the new nominees to every candidate in A who is not a nominee.

In the case where $m = 1$, the core of the EFA game contains at most one one-member slate $\{x\}$ where x is the candidate (if there is one) who would be chosen by the Condorcet criterion. So we know from Condorcet's paradox that the core of the EFA game may be empty in the case where $m = 1$. Similar paradoxes can occur for any value of m. For example, suppose that $m = 2$, $M = 4$ and $n = 20$, and that the profile of voters' preferences over candidates is:

Example 1. $n = 20$; $m = 2$; $q = 7$.

5 (1, . . . , 5): w, x, y, z
5 (6, . . . , 10): x, y, z, w
5 (11, . . . , 15): y, z, w, x
5 (16, . . . , 20): z, w, x, y

(This notation means that the five voters 1, . . . , 5 have the ordering w, x, y, z in strict descending order, the five voters 6, . . . , 10 have the ordering x, y, z, w, and so on.)

Notice that for every candidate there is another one such that the latter is preferred to the former by 15 voters, and that 15 voters are more than enough to secure the election of a complete slate. Thus every slate is blocked. (For example, the slate $\{w,x\}$ is blocked because 15 voters prefer the slate $\{x,z\}$.)

When $m = 1$, the core cannot contain more than one slate. But this result does not generalize. Here is a simple example where the core contains two slates:

Example 2. $n = 29$; $m = 2$; $q = 10$.

15 (1, . . . , 15): y, x, z
14 (16, . . . , 29): y, z, x

Of the three possible slates of two candidates, only $\{x,z\}$ can be blocked.

It is well known that in the case where $m = 1$, if voters' preferences are single peaked, there is one (and only one) candidate whose election satisfies the Condorcet criterion, and this candidate is the one who is the first preference of the median voter (Black, 1958, ch. 4). I shall now show that this result can be generalized.

Let me begin with a definition of single peakedness.

> *Single-peakedness.* Voters' preferences are single peaked if and only if there exists some strict ordering L on the set of candidates S such that for any three candidates x_1, x_2, x_3, if $x_1 L x_2 L_3$, then no voter prefers both x_1 and x_3 to x_2.

For the rest of this section I shall assume that preferences are single peaked. In principle it is possible that several L-orderings might exist—that a given profile of voters' preferences might be single peaked along several different dimensions. For the purpose of my argument it is necessary to select *one* such ordering (it does not matter which); this ordering will be called the *political spectrum,* and the relation L will read as "lies to the left of" or "is more left wing than." I shall now adopt the following notation.

> *Indexing of voters.* Voters are indexed by the integers $1, \ldots, n$ such that (i) no two voters share the same integer, and (ii) for any two voters, i, j where $i < j$: *either* i's most preferred candidate is more left wing than j's *or* i and j share the same first preference candidate.

These index numbers describe voters' positions on the political spectrum: the more left wing is a voter's first preference candidate, the lower is that voter's index number. To the extent that a group of voters share the same first preference candidate, the numbering of the voters is arbitrary; nonetheless, for the purposes of my argument, *some* particular assignment of index numbers must be selected—arbitrarily if necessary. Notice, however, that although the identity of "voter i" is in some degree arbitrary, the identity of "the candidate who is the first preference of voter i" is not (once the political spectrum is given). This allows the following notation:

> *The candidates* $C_1, \ldots, C_m$. For all $i = 1, \ldots, m$, C_i is the candidate whom voter iq most prefers.

Black's median voter theorem can now be formulated in the context of EFA: if $m = 1$, and if voters' preferences are single peaked, then the core of the EFA game contains just one slate, and the single candidate who constitutes this slate is C_1, the candidate who is the first preference of voter q. (if $m = 1$, $q = (n + 1)/2$, so voter q is the median voter.) This theorem can be generalized as follows.

> *Theorem 1. The median voter theorem generalized.* If voters' preferences are single peaked, and if there are m distinct candidates $C_1, \ldots, C_m$, then the core of the EFA game contains just one slate, and this slate is made up of those m candidates.

The theorem is proved in the Appendix. An example may help both to illustrate the implications of the theorem and to suggest how the proof works.

> *Example 3.* $n = 8$; $m = 2$; $q = 3$.
>
> 3 (1, 2, 3): w, x, y, z
> 2 (4, 5): x, y, w, z
> 1 (6): y, x, z, w
> 2 (7, 8): z, y, x, w

In this case, voters' preferences are single peaked in relation to the spectrum $w L x L y L z$, and they are indexed accordingly, from voter 1 on the left to voter 8 on the right. According to the theorem, the core of the EFA game should contain just one slate, $\{w,y\}$. (Since $q = 3$, the core slate should contain the candidates who are the first preferences of voters 3 and 6.) It is easy to see why any slate that excludes either w or y can be blocked. Any slate that excludes w can be blocked by a coalition of voter 3 and all those voters to the left of him—that is, voters 1, 2, and 3. These voters prefer w to all other candidates and have the power to elect him. Any slate that excludes y can be blocked either by a coalition of voter 6 and all those voters to the right—that is, voters 6, 7, and 8—or by a coalition of voter 6 and all those voters to the left—that is, voters $1, \ldots, 6$. For example, the slate $\{w,x\}$ can be blocked by voters 6, 7, and 8 nominating y; the slate $\{w,z\}$ can be blocked by voters $1, \ldots, 6$ nominating w and y. The formal proof of the theorem generalizes this basic idea and establishes that a slate such as $\{w, y\}$—in general, the slate $\{C_1, \ldots, C_m\}$—cannot be blocked by any coalition.

The theorem contains a clause about the candidates $C_1, \ldots C_m$ being *distinct;* the point of this clause can be illustrated by Example 2. In the example preferences are single peaked along the spectrum $x L y L z$, and the voters are indexed in a way that is consistent with this spectrum. Since $q = 10$, C_1 and C_2 are the first preferences of voters 10 and 20, respectively. Thus C_1 and C_2 refer to the same candidate, y, and the theorem does not apply. This is a case in which, despite voters' preferences being single peaked, there is no unique core slate.

At first sight Theorem 1 may seem paradoxical. It says that the core of the EFA game contains just one slate and that the candidates who make up this slate can be identified by their positions on the political spectrum L. But I have already said that voters' preferences might be single peaked in relation to more than one spectrum and that the choice of a particular spectrum for indexing candidates might be arbitrary. How can the core of the game depend on the arbitrary definition of the political spectrum? This paradox can be resolved only if it is a logical truth that whichever spectrum is used to index voters, the candidates who form the set $\{C_1, \ldots, C_m\}$ remain the same. That this is indeed a logical truth is proved in the Appendix.

Theorem 1 has an interesting implication. In effect it predicts that EFA will choose an assembly made up of the candidates $C_1, \ldots, C_m$. Now suppose that within the assembly decisions are taken by majority vote (and that there is an odd number of seats in the assembly). Then, applying Black's median voter theorem, the measures that the assembly passes will be those favored by the median representative. But the median representative is $C_{(m+1)/2}$, and (since $q = (n+1)/(m+1)$) this is the candidate who is the first preference of voter $(n + 1)/2$, the median voter. In a direct democracy the preferences of the median voter would prevail; in a representative democracy based on EFA, the median voter's preferences still prevail, although at one remove. Thus if direct democracy can be taken as a datum for evaluating systems of representation, EFA produces a representative assembly that can be said to be unbiased. This ties in with the argument about the double counting of majorities in the first section. Such double counting, which proponents of proportional representation have criticized, is one instance of the kind of bias that EFA may be able to avoid.

The Single Transferable Vote

Andrae, Mill, Hare, and (more recently) Lakeman have all appealed to the principle of free association as a justification for STV, but that principle can be put into practice most directly and most naturally in the form of EFA. What is the connection between EFA and STV? One way of answering this question is to consider some objections that might be made against EFA.

EFA is a cooperative game. It is a vital part of the procedure that each voter takes account of what the others are doing and that there is scope for any voter or group of voters to bargain with and coalesce with any other individual or group. Voters are *expected* to act strategically and not simply to nominate their most preferred candidates, irrespective of the plans of other voters. This is foreign to a long tradition of disapproval of strategic voting. (Recall Borda's famous remark about designing voting systems for honest men.) It might be objected that to make elections into games of strategy is to give an unfair advantage to those voters who happen to be most skilled at such games. In addition, elections of this kind are likely to be costly and time-consuming, for it may not be easy for voters to seek out those people who share their opinions. Thus wealthier voters may be at an advantage. Finally, EFA and secrecy are incompatible. How can people freely seek out those who share their opinions without publicizing what their opinions are? To anyone who is committed to the principle of the secret ballot, this is a very serious objection to EFA. (Neither Hare nor Mill would have seen this as a strong objection, since they were both strongly opposed to the secret ballot: cf. Hare, 1873, ch. 7; Mill, 1861, ch. 10.)

One way of overcoming these objections is to design a voting system that allows voters to record their preferences in secrecy and then *simulates* the bargaining and coalition forming that would take place under EFA. Then voters would not have to express their opinions in public and would be relieved of the burdens of thinking strategically and of seeking out like-minded fellow voters. Voters would be called on to record their true preferences, and so the system would be designed for honest men rather than for strategists. If STV is to be justified in terms of the principle of free association, the most promising route seems to be to justify it as a simulation of EFA. And this, I believe, is roughly what Andrae, Mill, and Hare had in mind.

I shall now investigate how successfully STV simulates EFA. For the purposes of my analysis I shall simply assume that, under STV, voters record their true preferences. In other words, I shall ask whether STV elects those candidates who would have been elected by EFA had voters really had the preferences they recorded on their ballot papers. The possibility that voters might instead behave strategically raises some interesting questions about STV, but I shall not address those questions in this article. I shall assume that the number of voters satisfies Assumption 2 and that voters' preferences satisfy Assumptions 3 and 4, but I shall not (until the end of the article) invoke the assumption of single peakedness.

In an election by STV each voter records a strict ordering over as many candidates as he wishes. To simplify matters, however, I shall assume that each voter records a strict ordering over the whole set S of candidates. The ballot on which a voter records his ordering is his *vote.* The process of counting votes involves two main operations, *transfer of surpluses* and *exclusion.* I shall begin by analyzing the first of these operations.

To secure election a candidate must get q votes, where q is the Droop quota, that is, the smallest integer greater than $n/(m + 1)$. Any votes that a candidate has in excess of q are surplus to his requirements. The fundamental principle of the transfer of surpluses is that any given vote should, in the first instance, go to the voter's first preference, but if that candidate does not need the vote for his quota, it may go to the voter's second preference, and so on. This principle can be formulated by way of the following concepts.

> *Allocations.* An allocation of votes is an exhaustive distribution among candidates of all the votes cast.
>
> *Election.* A candidate is elected (with respect to a given allocation of votes) if and only if he is allocated at least q votes.
>
> *Validity.* An allocation is valid if and only if no candidate is allocated more than q votes, *and* for all voters i and for all candidates x, y, if i prefers x to y and if x is not elected, then i's vote is not allocated to y.

The operation of transferring surpluses is the operation of arriving at some valid allocation. This is the first stage of any election by STV. If that valid allocation has the property that exactly m candidates are elected, the process of vote counting is at an end; those m candidates make up the slate of elected representatives.

The idea of transferring surpluses has a rationale in relation to the principle of simulating EFA, as the following theorem shows.

> *Theorem 2. Valid allocations and the core.* If there exists any valid allocation such that m candidates $x_1, \ldots, x_m$ are elected, then the slate $\{x_1, \ldots, x_m\}$ is in the core.

This theorem is proved in the Appendix. Thus, if the operation of transferring surpluses succeeds in electing m candidates, that slate of m candidates is a member of the core.

Theorem 2 does not involve any assumptions about the procedure by which a valid allocation is accomplished. This is significant, because it may be possible to allocate a given set of votes in various ways, all of which are valid; and different valid allocations may elect different candidates.

Example 4. $n = 9$; $m = 4$; $q = 2$.

5 (1, 3, 5, 7, 9): x, z, v, w, y
4 (2, 4, 6, 8): y, z, w, v, x

The modern conventions of STV (set out in detail in Lakeman & Lambert, 1955, Appendix 4) would work as follows. The first step is to count first preferences; x (15 votes) and y (4 votes) both exceed the quota, so x and y are elected on the "first count." Candidate X has the larger surplus, so his surplus (3 votes, e.g., those of voters 5, 7, and 9) is transferred to z. Now z (3 votes) has exceeded the quota and is elected on the "second count." The largest surplus outstanding is y's, and so this surplus (2 votes, say those of voters 6 and 8) is transferred to w. (The surplus is not transferred to z because z has already been elected.) Now w (2 votes) has just reached the quota and is elected on the "third count." Logically, the final stage (which in practice is omitted as unnecessary) would be to transfer z's surplus (1 vote) to v. The result of these transfers is the valid allocation (v: 9; w: 6,8; x: 1,3; y: 2,4; z: 5,7), i.e., candidate v has been allocated the vote of voter 9, candidate w the votes of voters 6 and 8, and so on. So conventional STV would elect the slate $\{w,x,y,z\}$. A different and rather simpler procedure for transferring surpluses was proposed by Carl Andrae (Andrae, P., 1926, pp. 4-5), whose method is to put all the ballot papers into a container and then to draw them out at random. Each vote is allocated to the first preference of the voter concerned unless that candidate has already reached the quota, in which case the vote is allocated to the voter's second preference (or if he too has reached the quota, to the voter's third preference, and so on). In the example, if votes were drawn out in numerical order, the result would be the valid allocation (v: 7,9; w: 8; x: 1,3; y: 2,4; z: 5,6), so Andrae's method would elect the slate $\{v,x,y,z\}$.

Theorem 2 establishes that where two different procedures for transferring surpluses would elect two different slates of candidates, both slates are in the core. To this extent, then, any particular procedure is arbitrary, although the principle underlying the whole family of procedures is not.

Unfortunately, it is quite possible that no valid allocation of votes will elect m candidates.

Example 5. $n = 9$; $m = 1$; $q = 5$.

4 (1, 2, 3, 4): x, y, z
2 (5, 6): y, z, x
3 (7, 8, 9): z, y, x

In this case no candidate is the first preference of q voters, so the question of transferring surpluses does not arise. The only valid allocation is (x: 1,2,3,4; y: 5,6; z: 7,8,9); no candidate is elected. However, the core is not empty: it contains one slate, namely $\{y\}$. (Voters' preferences are single peaked in relation to the spectrum $x\ L\ y\ L\ z$; y is the first preference of the median voter, voter 5.) This example establishes that for a slate to be in the core, it is not *necessary* that the slate can be elected on the basis of some transfer of surpluses. (This condition is, of course, sufficient; that is Theorem 2.)

When the transfer of surpluses produces a valid allocation that fails to elect m candidates, STV falls back on the second operation, exclusion. The candidate who has been allocated least votes is excluded from the contest, and his votes are transferred.

The principle here is to divide the votes among the remaining candidates in a way that *would have* constituted a valid allocation, had the excluded candidate not been standing for election. If after excluding one candidate and arriving at such an allocation, there are still fewer than m elected candidates, another candidate—the one who has been allocated least votes—is excluded; and so the process goes on until m candidates are elected.

It has occasionally been suggested that the operation of exclusion can lead to perverse results (Doron & Kronick, 1977). My analysis confirms this suggestion. Because of exclusion, STV can elect a non-core slate even though a core slate exists. Consider Example 5 again, in which STV would exclude y after the first count had failed to elect any candidate; y's votes would then be transferred to z, who would thereby reach the quota and be elected. But $\{y\}$ is in the core and $\{z\}$ is not. Similar problems can arise when m is greater than 1, as Example 3 shows. In this example $m = 2$, preferences are single peaked, and there is a unique core slate, $\{w,y\}$. STV would elect candidate w at the first count, but there would be no surplus votes to transfer. None of the other three candidates would reach the quota, and so y would be excluded, and his one vote would be transferred to x; the result would be the election of the slate $\{w,x\}$.

It is now possible to explain how successfully STV simulates EFA. Insofar as STV relies on the transfer of surpluses, it simulates EFA; in all cases where STV relies only on this operation, it chooses a slate from the core. But STV's other operation, exclusion, does not have a firm justification in terms of EFA.

This prompts a further question: is there any way of modifying STV so as to do away with the need for exclusion? One possible line of inquiry is suggested by the observation that if there are only $m + 1$ candidates, every valid allocation of votes elects exactly m candidates. (Because of Assumption 2, $q = (n + 1)/(m + 1)$; it follows immediately that every valid allocation of n votes among $m + 1$ candidates must give $q - 1$ votes to one candidate and q votes to each of the others.) It is therefore possible to do without the operation of exclusion by means of a succession of contests by STV, each of which involves only $m + 1$ candidates. After each contest, the loser drops out of the election, whereas the winning m candidates go forward into further contests, rather in the style of a knockout sporting tournament. If M candidates stand for election, a succession of $M - m$ such contests will leave only m candidates undefeated; these could then be deemed to be elected. I shall call this procedure *knockout voting by STV.* Notice that in the special case in which $m = 1$, knockout voting by STV is equivalent to a sequence of pairwise contests by simple majority vote, a procedure that is bound to elect the candidate (if there is one) who satisfies the Condorcet criterion.

Unfortunately, if $m > 1$ this procedure does not necessarily choose the core slate. The problem is this. The set of candidates is S. Suppose that, in relation to a contest among all these candidates, one slate, A, is in the core and that another slate B is not. Now consider any subset of candidates $S' \subset S$ such that S' contains both A and B. Clearly A must be in the core in relation to a contest between the candidates in S'. (This follows immediately from the concept of blocking described above.) But, *as far as the contest between the candidates in* S' *is concerned,* B may be in the core as well. If it is, then B rather than A might be elected by STV from S'. An example of this kind is presented in the Appendix. There are four candidates, w, x, y, z for two seats; only one slate, $\{x,z\}$, is in the core when the contest is among all four candidates. But in a contest among the three candidates x, y, z, the standard rules of STV would elect x and y. Thus the procedure of knockout voting by STV might fail to elect z, even though z is a member of the only slate in the core.

However, this problem will not arise if voters' preferences are single peaked. If preferences are single peaked, there is a unique core slate; in terms of the notation introduced above, this slate is $\{C_1, \ldots, C_m\}$ (see Theorem 1). The following theorem can be proved.

> *Theorem 3. Single peaked preferences and knockout voting by STV.* If voters' preferences are single peaked, then in any contest by STV between exactly $m + 1$ candidates drawn from S and given any valid allocation of votes, any candidate in the contest who is a member of the set $\{C_1, \ldots, C_m\}$ will be elected.

A proof is given in the Appendix. The theorem establishes that none of the candidates $C_1, \ldots, C_m$ can be defeated in any contest between $m + 1$ candidates by STV. Thus a succession of such contests must end in the election of the slate $\{C_1, \ldots, C_m\}$. So in the case of single peaked preferences, knockout voting by STV will always choose the core slate, even though (as Examples 3 and 5 show) the direct use of standard STV rules may not.

Conclusions

This article suggests an answer to the problem posed by Black: is there a unitary principle underlying proportional representation? I have presented a principle of free association, that electors should be free to choose the constituencies to which they belong. Given this principle, schemes of proportional representation are justified to the extent that they simulate elections based on free association. This idea can be expressed in terms of the core of a particular kind of game: my criterion for evaluating schemes of proportional representation is that, so long as the core of that game is not empty, the set of candidates who are elected should be a member of the core.

I have shown that this criterion yields unambiguous recommendations if voters' preferences are single peaked; that STV may fail to satisfy this criterion even under the assumption of single peakedness; and that a modified form of STV would satisfy the criterion under that assumption. Thus Carl Andrae, Thomas Hare, and John Stuart Mill were partly—but only partly—right when they appealed to the principle of free association as a justification for STV.

Appendix

A Proof of Theorem 1

Part (a) (To prove that any slate excluding one or more of $C_1, \ldots, C_m$ can be blocked.) Let A be any slate that excludes at least one of the candidates $C_1, \ldots, C_m$. Thus there must be some integer k (where $1 \leqslant k \leqslant m$) such that $C_k \notin A$. Consider any such candidate C_k. The slate A can be partitioned into two subsets, L and R, where L contains all those candidates in A who are more left wing than C_k, and R contains all those candidates in A who are more right wing than C_k. Let the number of candidates in L be j. First suppose that $j < k$. Because preferences are single peaked, each of the voters $1, \ldots, kq$ must prefer C_k to every candidate in R. These voters, kq in number, have the power to elect k candidates, where $k \geqslant j + 1$. Thus the coalition of left-wing voters $1, \ldots, kq$ can block A by nominating all the candidates in L, *plus* C_k. Now suppose instead that $j \geqslant k$. Each of the voters $kq, \ldots, n$ prefers C_k to every candidate in L. These voters, $(m - k + 1)q$ in number, have the power to elect $m - k + 1$ candidates, where $m - k + 1 \geqslant m - j + 1$. The number of candidates in R cannot be greater than $m - j$. Thus the coalition of right-wing voters $kq, \ldots, n$ can block A by nominating all the candidates in R, *plus* C_k.

Part (b) (To prove that $\{C_1, \ldots, C_m\}$ cannot be blocked.) Suppose that the slate $B = \{C_1, \ldots, C_m\}$ is blocked. Then there must exist p "repeat" nominees ($m > p \geqslant 0$) drawn from B *and* r "new" nominees ($m \geqslant r > 0$) drawn from $S - B$ *and* a coalition of $(p + r)q$ voters, each of whom prefers at least one of the r new nominees to every candidate in B who is not a nominee. Let those candidates in B who are not nominees be called *un-nominated candidates* (*UC*s). Now consider which voters can be in the blocking coalition. For each *UC*, say C_i, there is one voter, namely iq, who cannot possibly be in the blocking coalition, since we know that his first preference candidate is C_i. Since there are $m - p$ *UC*s, we can eliminate $m - p$ voters from the class of possible members of the coalition. These voters can be called *eliminated voters.* The remaining voters can now be partitioned into $m - p + 1$ sets defined in relation to the eliminated voters. If iq is the most left wing of the eliminated voters, the first set, V_1 is $\{1, \ldots, iq-1\}$. If jq is the secondmost left wing of the eliminated voters, the second set, V_2, is $\{iq+1, \ldots, jq - 1\}$, and so on until the final set, V_{m-p+1}, which is $\{kq+1, \ldots, n\}$, where kq is the most right wing of the eliminated voters. Similarly, if we exclude the *UC*s from the set of candidates S, the remaining candidates can be partitioned into $m - p + 1$ sets defined in relation to the *UC*s: the first set, W_1, contains all those candidates who are more left wing than the most left-wing *UC*, the second set, W_2, contains all those candidates occupying places on the political spectrum between the most left-wing *UC* and the secondmost left-wing *UC*, and so on.

Now consider any set of voters, V_l, and the corresponding set of candidates, W_l. Because preferences are single peaked, one of the following must be true: *either* W_l contains at least one new nominee *or* no voter in V_l is a member of the blocking coalition. (First consider the set of voters $V_1 = \{1, \ldots, iq-1\}$. All of these voters prefer C_i, who is a *UC*, to every more right-wing candidate, so if there is no new nominee to the left of C_i, i.e., no new nominee in W_1, these voters cannot be in the blocking coalition. Similarly for $V_2 = \{iq+1, \ldots, jq-1\}$: all of these voters prefer C_i to every more left-wing candidate and prefer C_j to every more right-wing candidate, and so if there is no new nominee between C_i and C_j on the spectrum, *i.e.*, no new nominee in W_2, these voters cannot be in the blocking coalition. And so on.)

Since there are only r new nominees, no more than r of the sets of candidates $W_1, \ldots, W_{m-p+1}$ can contain a new nominee. Thus the voters in the blocking coalition can be drawn from no more than r of the sets $V_1, \ldots, V_{m-p+1}$. Consider any set of voters V_l. If the number of repeat nominees in W_l is s, the number of voters in V_l is $(s + 1)q - 1$. What is significant is that this number is less than $(s + 1)q$. There are only p repeat nominees in all, and the members of the blocking coalition can be drawn from no more than r sets; so the total number of voters in the blocking coalition must be less than $(p + r)q$. But this contradicts the original supposition that the coalition contains at least $(p + r)q$ voters.

A proof that the slate $\{C_1, \ldots, C_m\}$ is uniquely defined

A candidate will be said to be *supported* if he is the first preference of at least one voter. Notice that nonsupported candidates are irrelevant as far as the definition of the slate $\{C_1, \ldots, C_m\}$ is concerned, since $C_1, \ldots, C_m$ are supported candidates by definition, and the indexing of voters is based only on the position on the political spectrum of voters' *first-preference* candidates. Let T be the subset of S that contains all supported candidates. I shall now prove that if voters' preferences on the set S are single peaked, there are only two ways of ordering the candidates in T to form a political spectrum, and one is the exact inverse of the other. In other words if $x_1 \, L \, x_2 \, L \ldots L \, x_k$ is an ordering of the k candidates in T that is consistent with single peakedness, the only other ordering that is similarly consistent is $x_k L \ldots L \, x_2 \, L \, x_1$.

Suppose that voters' preferences are single peaked along some spectrum. Then in the set T (provided it contains at least two candidates), there must be two candidates who lie at the extremes of this spectrum, and these are the only candidates who can be ranked "worse in T" by any voter. But since each of these candidates is some voter's first preference, each of them must be ranked worst by someone. Thus there are two and only two candidates who are ranked "worst in T" by any voter, and these candidates must lie at the extremes of *any* spectrum along which preferences are single peaked. Suppose that one of these two candidates, say x_1, is arbitrarily called the most left wing; the other, say x_k, is then the most right wing. Now remove x_1 from T, leaving the set T'. By the same argument, if T' contains two or more candidates, there are exactly two candidates who are ranked "worst in T'" by any voter. One of these must be x_k; the other, say x_2, must be the second most left-wing candidate in T. Continuing like this, one eventually arrives at the spectrum $x_1 L x_2 L \ldots L x_k$. If instead x_k had been called the most left-wing candidate, one would have arrived at the spectrum $x_k \, L \ldots L \, x_2 \, L \, x_1$. Whichever of these two spectra is used, provided that $q = (n + 1)/(m + 1)$, the slate $\{C_1, \ldots, C_m\}$ is the same. (If a candidate is called C_1 in terms of one spectrum, he is called C_m in terms of the other; if he is called C_2 in terms of one, he is called C_{m-1} in terms of the other; and so on.)

A Proof of Theorem 2

Suppose that $x_1, \ldots x_m$ are elected for some valid allocation Q but that the slate $A = \{x_1, \ldots, x_m\}$ can be blocked. Then there must be p "repeat" nominees, say $x_1, \ldots, x_p$, drawn from A, and r "new" nominees $y_1, \ldots, y_r \, (r \geqslant 1)$ drawn from $S - A$, and a coalition of $(p + r)q$ voters such that each member of the coalition prefers one of $y_1, \ldots, y_r$ to all of $x_{p+1}, \ldots, x_m$. By assumption, none of $y_1, \ldots, y_r$ is elected, given the allocation Q. Therefore $x_{p+1}, \ldots, x_m$ are allocated no votes belonging to coalition members. Since $r \geqslant 1$ and $q > n/(m + 1)$, the number of voters in the coalition is greater than $n(p + 1)/(m + 1)$; thus the number of votes allocated to $x_{p+1}, \ldots, x_m$ is less than $n - n(p + 1)/(m + 1)$, i.e., less than $(m - p)[n/(m + 1)]$. But this in turn is less than $(m - p)q$. If $x_{p+1}, \ldots, x_m$ are elected, they must be allocated exactly $(m - p)q$ votes. The original supposition therefore entails a contradiction.

An example in which knockout voting by STV does not choose a core slate

$n = 101; m = 2; q = 34$

32 (1, . . . , 32): x, y, z, w
30 (33, . . . , 62): w, x, y, z
5 (63, . . . , 67): z, w, x, y
34 (68, . . . , 101): x, z, y, w

Notice that $(n + 1)/(m + 1) = 34$; so Assumption 2 holds for this example. Any slate that excludes x is blocked because more than q voters prefer x to all other candidates. $\{x,y\}$ is blocked because more than q voters prefer w to both x and y. $\{x, w\}$ is blocked because more than $2q$ voters prefer z to w. But $\{x,z\}$ cannot be blocked: it is, therefore, the only slate in the core. Now suppose that there is knockout voting by STV and that the first contest is to choose two candidates from the set $\{x,y,z\}$. Voting would be as follows:

$n = 101; m = 2; q = 34$

62 (1, . . . , 62): x, y, z
5 (63, . . . , 67): z, x, y
34 (68, . . . , 101): x, z, y

x has 96 first preference votes and so must be elected. But which of y and z is elected depends on how x's 62 surplus votes are distributed. The standard STV procedure (Lakeman & Lambert, 1955, Appendix 4) would divide these between y and z in the ratio 62:34, thus giving 40 votes to y. This is well in excess of the quota, and so the slate $\{x,y\}$ would be chosen, and z would drop out. Obviously, then, the outcome of the succession of contests would not be the election of the core slate $\{x,z\}$.

A Proof of Theorem 3

Consider any contest among $m + 1$ candidates, at least one of whom, e.g., C_k, is a member of the set $\{C_1, \ldots, C_m\}$. Let the remaining candidates be denoted $x_1, \ldots, x_m$; thus the set of contestants is $\{x_1, \ldots, x_m; C_k\}$. It follows from part (a) of the proof of Theorem 1 that the slate $\{x_1, \ldots, x_m\}$ can be blocked and therefore is not in the core. Thus, by Theorem 2, there is no valid allocation of votes such that all the candidates $x_1, \ldots, x_m$ are elected. But because there are exactly $m + 1$ candidates in the contest, every valid allocation of votes elects m candidates. Therefore every valid election elects C_k.

Generalizing the analysis for any number of voters

If Assumption 2 is dropped, n can be any integer greater than m, and q can be defined as $n/(m + 1) + \delta$ where $n/(m + 1) + \delta$ is an integer and $1/(m + 1) \leqslant \delta \leqslant 1$. (Assumption 2 then corresponds with the special case $\delta = 1/(m + 1)$.) Dropping this assumption does not affect anything of substance in the article until the candidates $C_1, \ldots, C_m$ are defined.

Candidate C_i is defined as the first preference of voter iq. As I have proved earlier in this Appendix, the set of candidates $\{C_1, \ldots, C_m\}$ is uniquely defined and is independent of which end of the spectrum is called "left" and which "right," *provided that Assumption 2 holds.* Given that assumption, if voters are numbered $1, \ldots, n$ where 1 is "left" and n is "right," the (iq)th voter counting from the left is also the $[(m + 1 - i)q]$th voter counting from the right. In general, however, this may be not the case. The $[(m + 1 - i)q]$th voter counting from the right is voter $iq - [\delta(m + 1) - 1]$, and this is not necessarily voter iq, since $\delta(m + 1) - 1$ may be positive rather than zero. This problem can be avoided by defining a set of voters H_i where $H_i = \{iq - [\delta(m + 1) - 1], \ldots, iq\}$. In other words, H_i is the intersection of the set containing the first iq voters, counting from the left, and the set containing the first $(m + 1 - i)q$ voters, counting from the right. The number of voters in this set is $\delta(m + 1)$, and we know that $1 \leqslant \delta(m + 1) \leqslant m + 1$. Assumption 2 represents the special case where $H_i = \{iq\}$ for all $i = 1, \ldots, m$.

Here an example may help. Let $n = 9$ and $m = 2$; thus $q = 4$ and $\delta = 1$. H_1 is the intersection of the set containing the first 4 voters, counting from the left, i.e., $\{1, \ldots, 4\}$ and the set containing the first 8 voters, counting from the right, i.e., $\{2, \ldots, 9\}$; thus $H_1 = \{2,3,4\}$. Similarly, $H_2 = \{6,7,8\}$. Notice that the number of voters in each set is 3, where $3 = \delta(m + 1)$.

The following theorem, which does not depend on Assumption 2, can be proved in essentially the same way as Theorem 1, and entails Theorem 1 as a special case:

> *Theorem 1A.* If voters' preferences are single peaked, *and* for all $i = 1, \ldots, m$, all the voters in H_i share a common first preference candidate C_i, *and* $C_1, \ldots, C_m$ are distinct candidates, then the core of the EFA game contains just one slate, namely $\{C_1, \ldots, C_m\}$.

Theorem 1 is silent unless $(n + 1)/(m + 1)$ is an integer; Theorem 1A is silent unless all the voters in each set H_i share a common first preference candidate. How great an advance is this? If the number of voters, n, is large in relation to both the number of seats, m, and the number of candidates, M, then the advance is considerable. Notice that the number of voters in any set H_i is never greater than $m + 1$, and that the voters in such a set are (in an intuitive sense) located next to one another on the political spectrum. Thus if the total number of voters is large and the total number of candidates is small, the probability is high that all the voters in a set H_i will share a common first preference candidate; for any given m and M, this probability approaches 1 as the number of voters approaches infinity. Elections, particularly the most significant elections in national political life, typically involve a large number of voters and a relatively small number of candidates; in such cases, Theorem 1A is highly likely to apply.

The first part of the third section, up to the start of the discussion of knockout voting, is independent of Assumption 2. In particular, Theorem 2 holds for any number of voters. However, if Assumption 2 does not hold, it is possible for a valid allocation of votes to fail to elect m candidates, even if only $m + 1$ candidates are standing for election. (For example, suppose that n is a multiple of $m + 1$; then $q = n/(m + 1) + 1$. If first preference votes are divided equally between the $m + 1$ candidates, each candidate will receive $n/(m + 1)$ first preference votes, and none will reach the quota.) Despite this apparent problem, Theorem 3 can be reformulated in much the same way as Theorem 1:

> *Theorem 3A.* If voters' preferences are single peaked *and* for all $i = 1, \ldots, m$, all the voters in H_i share a common first preference candidate C_i, then in any contest by STV between exactly $m + 1$ candidates drawn from S, any

candidate in the contest who is a member of the set $\{C_1, \ldots, C_m\}$ will be elected.

The proof is straightforward. Let D be any set of $m + 1$ candidates, and suppose that for some integer k (where $1 \leqslant k \leqslant m$), the candidate C_k is a member of D. Suppose also that there is some valid allocation of votes that does not elect C_k. Because preferences are single peaked, each of the voters $1, \ldots, kq$ must prefer C_k to any more right-wing candidate, so none of these kq votes can have been allocated to any candidate who is more right wing than C_k. But no candidate has been allocated more than q votes, and C_k has been allocated fewer than q votes. This is possible only if there are at least k candidates in D who are more left wing than C_k. But similarly, each of the $[m + 1 - k]q$ most right-wing voters must prefer C_k to any more left-wing candidate, from which it follows that there must be at least $m + 1 - k$ candidates in D who are more right wing than C_k. These two results together entail that D must contain at least $m + 1$ candidates in addition to C_k, and this contradicts the supposition that D contains only $m + 1$ candidates in all. Thus any valid allocation of votes must elect C_k.

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Advocacy, Interpretation, and Influence in the U.S. Congress

RICHARD A. SMITH
Carnegie-Mellon University

This article examines the relationship between two variables: interpretations by members of Congress of the consequences of legislative proposals and the influence of those who advocate particular actions on the proposals. The article investigates how the legislative decisions of members depend on interpretations, how the arguments of advocates shape interpretations, and how the influence on interpretations translates into influence over patterns of congressional support expressed for a legislative proposal. The major thesis is that member interpretations and hence legislative influence are unstable, a result of basic features of human decision making in the congressional context. As a result, advocates are hypothesized to have considerable influence in maintaining and expanding the size of their congressional coalitions in some legislative situations, but not others. In particular, when faced with weakening amendments or motions, advocates are unable to prevent defections among their supporters. Evidence of the legislative influence of the National Education Association during the Ninety-fourth Congress is found to be consistent with these hypotheses.

In the past decade, most explanations of legislative decision making in Congress have been teleological. Members of Congress are described as seeking a variety of personal goals. For example, the goals may pertain to the realization of their conceptions of good public policy, the development of their careers both in the chamber and back home, and the implementation of their views of representation. Legislative decisions are explained as the result of members pursuing these goals in accordance with their perceptions of a rich and complex decision environment, consisting of constituents, colleagues, party leaders, personal and committee staff aids, interest groups, bureaucrats, presidents, and journalists.[1]

Yet all these explanations share a common shortcoming: they devote little attention to the process by which members come to understand how a legislative decision may affect the achievement of their goals. The process involves interpretation and has two stages. The first stage consists of determining the consequences (and their associated probabilities of occurrence) of a legislative decision. Members must discover who will be affected by the legislative decision, in what ways, to what extent, and with what reaction. The discovery, however, is rarely complete: the quantity of information required is too large, the time available to attend is too limited, and the calculations required are too complex (e.g., Bauer, Pool, & Dexter, 1972, pp. 408-413; March & Simon, 1958, pp. 150-158). To simplify the decision task, members construct interpretations of the consequences of a decision. These interpretations are simple models or representations of the more complex ways in which a legislative decision may affect the lives of citizens and the institutional and electoral careers of the members. The second stage consists of relating these interpretations to the personal goals of members; that is, once an interpretation of consequences is formulated, members must then discover how the consequences affect the attainment of their personal goals. This is not a straightforward task. Because of cognitive limitations, the goals of members often are formulated in ways that lack precision. It is difficult to make goals reliably operational in the evaluation of different consequences (Bauer, Pool, & Dexter, 1972, chap. 25; March, 1978, p. 596). As a result, the discovery of how consequences affect goal attainment is also partial. Members develop interpretations of the impact.

This article explores some of the implications of this two stage interpretation process for coalition

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[1] The research is too extensive to cite in its entirety. But notable examples are Arnold (1979), Fenno (1973, 1978), Fiorina (1977), Mayhew (1975), and Price (1978).

building on the floors of the Congress. Its main objective is to explain the relationship between advocacy—the presentation of arguments in support of a legislative proposal—and influence over the decisions of Congress. The article advances two central arguments. First, the influence of advocates, whether they be presidents, interest group lobbyists, or members of Congress, depends on the interpretation process. Second, the influence of advocates is unstable; advocates can exert substantial influence on congressional floor decisions only to see that influence dissipate in crucial legislative situations. These arguments are developed by examining three questions: how do the legislative decisions of members of Congress depend on interpretations, how do the arguments of advocates shape these interpretations, and, finally, how does the influence over interpretations translate into influence over the patterns of congressional support expressed for a legislative proposal?

The article is divided into two parts. The first part is a theoretical discussion of the relationship among advocacy, interpretation, and influence. The discussion has its origins in research designed to understand how one type of advocate—interest group lobbyists—influences congressional decisions. During the spring and summer of 1977, the lobbying activities of the National Education Association were observed in detail, and extensive, open-ended interviews were conducted with members of the government relations staff. Since these observations and interviews provided detailed information about how one kind of advocate tried to establish and maintain a relationship with the Congress for the purpose of influencing legislation, they served as a basis for thinking about how efforts by any advocate to influence legislation might actually affect congressional decisions. The theoretical discussion that follows owes much to the observation and interviewing experience. However, since the discussion centers on the decision processes of members of Congress, little interview evidence is presented. Rather than using the perceptions of lobbyists to describe member decision processes, a far richer and more accurate description of the processes is developed by drawing on propositions from the behavioral theories of individual choice developed by cognitive and social psychologists, organizational theorists, and political scientists, especially students of Congress.

The second part is an empirical analysis. Its primary purpose is to provide evidence of influence by an advocate that is consistent with the theoretical relationship posited for advocacy, interpretation, and influence. To this end, two hypotheses about the nature of an advocate's influence on maintaining and expanding the size of its supporting congressional coalitions are tested with data provided by the National Education Association (NEA). The data concern the NEA's lobbying activities and legislative interests in the 94th Congress. At one time, comparative analyses of several interest groups were planned, but after preliminary investigations of nearly 20 interest groups, it was ascertained that only the NEA maintained the records necessary to determine the entire range of its legislative interests *and* gave the access necessary to collect and interpret the data. The latter consisted of members of the government relations staff making available written records, spending hours describing their relationships with staff assistants and members of Congress, and permitting unrestricted observation and questioning of their lobbying activities regarding different legislative proposals. Whether the empirical results for the NEA generalize to other interest groups and other kinds of advocates in other congresses can only be determined through further empirical analyses. But these initial results serve the important dual functions of demonstrating how to test some of the implications of the theoretical arguments of this article as well as establishing the plausibility of those arguments.

Interpretation, Decision Making, and Legislative Influence

The interpretation process occupies a central place in the legislative decisions of members, and hence the influence of advocates over those decisions. Its place can best be delineated by the discussion of five propositions.

1. For many legislative proposals, the positions of most members are not directly influenced by interpretations.

Because of both time and cognitive constraints, most members so simplify their decision task outside their areas of specialization that the interpretation process largely disappears. The simplification may take a variety of forms, but one of the most common is cue taking. Matthews and Stimson (1975, p. 45), for example, argue that "when a member is confronted with the necessity of casting a roll call vote on a complex issue about which he knows very little, he searches for cues provided by trusted colleagues who—because of their formal position in the legislature or policy specialization—have more information than he does and with whom he would probably agree if he had the time and information to make an independent decision." (For evidence, see Matthews and Stimson, 1975, and Kingdon, 1973, especially chaps. 3 and 4.) Yet because of the reliance on cue taking, the interpretation process can still have a major, although indirect, impact on the overall pattern of support for a legislative proposal. Interpretations are likely to be a critical variable in the decisions of many cue givers, especially those members who serve on the committees and subcommittees with jurisdiction over the legislative proposal. These members receive the greatest quantity of information about the consequences of the proposal, and they have the most time and interest to develop interpretations. Since their decisions will serve as cues to less informed and interested members, the interpretation process indirectly influences the decisions of many members. The process, therefore, can be a central variable in the dynamics of coalition formation, even though it generally has little apparent role in the decision processes of many members. Consequently, the remainder of this discussion focuses on those members whose positions are directly influenced by interpretations.

2. *When the positions of members are based on interpretations, they are rarely optimal in the sense of positions that would best accomplish the personal goals of members.*

The development of a position by members on a legislative proposal depends significantly on two variables: their personal understanding of how the position affects the achievement of their goals, and their public explanation of the position to interested audiences (e.g., constituents and other members of Congress) who can aid the members in the achievement of their goals (e.g., those regarding the development of a career within the chamber or at home or both). The aim of public explanations is to show how a position on a legislative proposal is consistent with the interests and preferences of interested audiences. If members believe they cannot successfully explain their position, they may seek an alternative position that is more publicly defensible (Fenno, 1978; Kingdon, 1973). Both personal understandings and public explanations depend critically on knowledge of the consequences of a position. Obviously members cannot understand how a position will affect their goals if they have no knowledge of its consequences. Nor can they explain why interested audiences should accept their position if they cannot show how the consequences of the position are consistent with the interests and preferences of the audiences.

The adoption of the optimal position on a legislative proposal would require complete knowledge of the consequences of alternative positions on a legislative proposal. Members would take the position with the set of consequences that they believed best contributed toward their personal goals and permitted explanations that were most likely to be accepted by interested audiences. However, members rarely take optimal positions. The major reason is that members do not know all the consequences of different decisions regarding a legislative proposal. Discovering the consequences is a complex problem, requiring the processing and evaluation of tremendous quantities of information about who is affected, in what ways, and to what extent. Because members are boundedly rational decision makers, they discover only a subset of the actual consequences of a proposal. These partial discoveries result in simplified models or interpretations of the full set of consequences. The positions and attendant explanations based on such interpretations can be considerably different from those given if the larger set of consequences were known.[2]

From a bounded rationality perspective, there are two reasons why members (and more generally decision makers) discover only a subset of the actual consequences of a proposal. First, the information that members acquire about the consequences of a proposal is the result of a limited search. The personal goals of members are typically expressed in the form of aspiration levels. (For a general discussion of the concept, see March, 1978; March & Simon, 1958; Simon, 1955.) Members process information and develop interpretations about the consequences of different positions regarding a legislative proposal until they believe a particular position will yield some acceptable or satisfactory level of accomplishment of their personal goals. Other positions may please more constituents, lead to better public policy, or result in greater influence within the chamber or perhaps all of these. But, because of time and cognitive constraints, members are content with a position that appears to be good enough. Members are satisficers rather than maximizers (Simon, 1955).

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[2] As Simon (1957, pp. 198-199) has argued about rational decision makers in general:

> The capacity of the human mind for formulating and solving complex problems is very small compared with the size of the problems whose solution is required for objectively rational behavior in the real world—or even for a reasonable approximation to such objective rationality. . . . The first consequence of the principle of bounded rationality is that the intended rationality of an actor requires him to construct a simplified model of the real situation in order to deal with it. He behaves rationally with respect to this model, and such behavior is not even approximately optimal with respect to the real world. To predict his behavior, we must understand the way in which this simplified model is constructed.

For extended discussions of this point, see Simon (1957), Part IV, especially pp. 196-206; and March and Simon (1958), especially pp. 136-171. For related experimental evidence, see Tversky and Kahneman (1981) on decision frames and Taylor and Crocker (1980) and Fiske and Linville (1980) on schematic information processing.

Second, members often ignore potentially relevant information about the consequences of a proposal. The oversight occurs because they simply do not perceive its relationship to the achievement of their personal goals. The poor perception is the result of cognitive constraints on human memory and information processing capacity. These constraints can cause the goals of members to be "unstable, inconsistent, incompletely evoked, and imprecise" or, in a word, ambiguous (March, 1978, pp. 593-599, especially p. 598). Or the constraints can make it difficult for members to see how disparate pieces of information fit together in a way that establishes some consequences of a proposal (Smith, 1980, pp. 71-93). In either case, members will fail to perceive the link between some of the consequences of a proposal and the achievement of their personal goals.

3. By offering various interpretations, advocates can show members how an alternative position may better accomplish their mixes of personal goals than the ones they currently espouse.

Advocates devote considerable effort to trying to structure how members process information about consequences and, therefore, construct interpretations. Indeed, a major portion of their coalition-building activities consists of presentations of arguments that establish various interpretations of the consequences of different positions on a legislative proposal. The aim is to show how the position favored by the advocate is also one consistent with the goals of the members—either by shaping the members' personal understandings of the consequences or by providing members with acceptable explanations of their positions.[3] As an example, consider the two presentations reproduced in Table 1. They concern an amendment offered by Robert Michel of Illinois to the FY1978 Labor HEW Appropriations bill (HR 7555). The amendment proposed trimming the committee approved bill by $563 million. Presentation A is a portion of a "Dear Colleague" letter written by Representative Michel. Presentation B is the text of a mailgram sent by the National Education Association to all members of the House. The two presentations are in striking contrast to one another. They emphasize different consequences (e.g., Michel raises the prospect of a presidential veto if his amendment is not adopted). And they disagree about others (e.g., Michel claims his amendment still provides generous support of federal programs, thereby making any cuts in regular school programs or increases in local property taxes unlikely).[4]

Such presentations are made at every stage of the congressional process, beginning with the drafting of a legislative proposal before its introduction in the House or Senate and ending with the final floor vote on the proposal. They are also made in a variety of ways, including testimony before congressional committees, conversations with members or their staff assistants, and letter and telegram campaigns. Presentations can influence the interpretations and decisions of members for two reasons. First, they structure how members search for information about consequences, and second, they can cause members to reconsider information that they may have overlooked in prior searches. Advocates appear to recognize the importance of making presentations. Both interest group lobbyists and members of Congress, for example, see presentations as the most important activity for influencing the decisions of Congress (Berry, 1977; Milbrath, 1959, 1963; Scott & Hunt, 1966).

However, the influence that results from presentations has one significant drawback for advocates: it can be very fragile. The very information-processing characteristics of members that give advocates the opportunities to influence the decisions of members also give opposing advocates similar opportunities. Since members form interpretations as a result of limited search and oversight, opposing advocates may have opportunities to overturn existing interpretations (and positions) by making members aware of alternative, more appealing interpretations. As an example, consider an incident reported by Eric Redman (1973, pp. 206-207), involving the Senate's con-

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[3] Fenno presents some examples of explanations that clearly suggest the place of interpretations in explanations. The explanations of two House members of their potentially unacceptable positions on the Voting Rights Act of 1970 are particularly illustrative (Fenno, 1978, pp. 151-152). For a more extended discussion of the relationship between interpretation and explanation, see Smith, 1980, pp. 93-99.

[4] Just how much either of these presentations actually influenced the interpretations and decisions of member is unknown. Michel's amendment was defeated by a vote of 334-72. Because of the protracted dispute over the provisions of federal funds for abortion, the appropriation levels established in conference report of H.R. 7555 were enacted into law in a continuing appropriation resolution (HJ Res. 662) approved in December of 1977. See Public Law 95-205.

Table 1. Two Presentations Regarding an Amendment to the FY 1978 Labor-HEW Appropriations Bill

Presentation A	Presentation B
As you are undoubtedly aware, the vast special interests communication network being what it is, I plan to offer an amendment to reduce the Labor-HEW Appropriations Bill by $563 million when the bill is taken up on the Floor on Wednesday, June 15. Enclosed is a table which shows the items in the bill, the dollar amounts, and various comparisons. There has been some concern expressed that this amendment would cut current program levels. That is *not* the case. As you will note from the table, in no case does the amendment reduce funding below the current (FY 1977) level. The one apparent reduction, in new capital contributions for the Direct Student Loan program, is more than offset by a $24 million increase in the Direct Loan revolving fund. Most importantly, in every instance but one, there would be an increase over the current funding level, in some cases a very substantial increase. In every case but two, the amendment would result in a funding level above the Carter budget. What the amendment would do, in other words, is basically just moderate some of the major increases in the bill. Since the bill is a substantial $2.1 billion above the Carter Budget, and in view of the President's talk about a possible veto of the bill, I believe we ought to seek to make the measure somewhat more reasonable in nature in order to head off a confrontation. My amendment treats each program in a fair and generous manner, and would reduce the total in the bill without hurting any of the programs involved. A vote for the amendment would be a vote for a more moderate bill and still for increases in the programs. I hope to have your support.	The National Education Association urgently needs your support of education funding as reported by the House Appropriations Committee in the FY 1978 Labor-HEW bill, H.R. 7555. Specifically, we ask your support of the committee recommendations—without amendments in the education sections. The rationale of our position is as follows. 1) The committee recommendation falls well within the target for education spending adopted in the first concurrent resolution on the budget. That target was approved by the full House May 17 after full debate and conference with the Senate. 2) Most of the programs in the Bill have been mandated by the Federal Courts and by Congress. Such programs as ESEA Title 1, Impact Aid, Education of the Handicapped, and Student Assistance, affect the vast majority of children and youth in school, directly or indirectly. Regular school programs will have to be trimmed if the major federal-funded programs are cut. 3) Inflation of school costs have been worsened by steeply rising energy prices. Without the full amounts recommended in H.R. 7555, local governments will have to increase property taxes drastically in order to maintain current programs. 4) The federal share of school funding must be stable and reliable. The Appropriations Committee figures represent the rock-bottom level of support needed to prevent certain disaster.

sideration of an amendment offered by Senator Warren G. Magnuson of Washington to block a proposed shipment of nerve gas from Okinawa through the Pacific Northwest. The amendment appears destined for defeat. Redman writes:

> I prepared a memorandum cataloging the arguments he (Magnuson) had marshalled against the shipment and took it to him at his desk on the Senate Floor. He surveyed the memo cursorily, then handed it back with annoyed "No, no, no!" Bewildered, I retreated to the staff couch and waited to hear what argument he intended to use instead of familiar ones of possible sabotage, dangerous sections of track along the proposed route, and populations that would have to be evacuated as a precaution against leakage. When the time came, he took a wholly novel and ingenious approach. The issue he told his colleagues, was not one of the people versus the Pentagon, as the news media seemed to assume. Instead, it was another case of the President versus the Senate. The Senator from West Virginia (Robert C. Byrd) had recently offered a resolution, which the Senate had passed, stating that the Senate expected the President to keep it informed throughout the treaty negotiations with the Japanese government on the subject of Okinawa. The President's sudden decision to move the nerve gas off Okinawa must reflect some aspect of those treaty negotiations, Magnuson insisted—and the Senate had not yet been informed of, much less consented to, any such agreement. To allow the nerve gas shipment under such circumstances, he asserted, would be to abandon the Byrd resolution and to abdicate the Senate's rightful role in treaty-making generally. The President, Magnuson said, might get the idea that he could ignore the Senate and its Constitutional prerogatives whenever he wished. Jolted by this reasoning, the Senator from West Virginia and his Southern colleagues —friends of the Pentagon almost to a man, but vigilant guardians of the Senate's Constitutional responsibilities—voted down the line with Mag-

> nuson. The amendment, which had been doomed a few minutes earlier, passed overwhelmingly.[5]

Magnuson's arguments seem to be influential because they provided an interpretation that demonstrated to some senators how an alternative position was superior to the one they currently held. The arguments were effective because they demonstrated the connections between pieces of disparate, seemingly unrelated information. His arguments provided no new information. The senators all knew about the Byrd resolution, they all knew about the President's decision to remove the nerve gas, but few if any senators saw the connection between the two and hence, the connection with the transportation of the nerve gas across the Pacific Northwest. Magnuson simply suggested how the two pieces of information were related, thereby allowing him to identify a set of consequences previously overlooked by senators. The result was the construction by many southern Democratic senators of the President-versus-the-Senate interpretation—one that apparently dominated their prior interpretations.

All of this is not to suggest that the positions and interpretations of all members are equally subject to manipulation. They are not. Manipulation is also a function of other variables. One of the most important is the level of agreement between advocates and members on previous legislative proposals: the higher the level of historical agreement, the greater the probability that the advocate will develop an interpretation that members will find appealing over all others. The level of historical agreement is important because it reflects a basic similarity in the goals (or at least relevant subsets of them) of advocates and members. For example, advocates and members may share similar conceptions of what constitutes good public policy, pursue common career ambitions, or profess corresponding views of representation. When the similarity in goals is high, the advocate finds that the member has similar preferences over the various consequences of different positions on a legislative proposal. It thus becomes relatively easy to develop interpretations that will elicit a position by the member that is desired by the advocate. And over time, the member will become a *regular supporter* of the advocate's positions. When the similarity in goals is low, the advocate will have many more conflicts with the member over the desirability of various consequences. It becomes far more difficult for the advocate to develop an interpretation of the consequences that will cause the member to take the position desired by the advocate. And over time, the member is likely to be only an occasional or *irregular supporter* of the advocate's positions.

4. The probability that advocates can demonstrate the superiority of their desired positions to members depends significantly on two variables: the level of resource advantage (i.e., the quality and quantity of resources committed to the advocacy effort compared to what opposing advocates have committed) and the amount of time available for the advocacy effort.

Consider first the notion of a resource advantage. In order to influence the interpretations and positions of members, advocates must develop interpretations that members find appealing and superior to those they already hold (if any). Advocates can control to some extent how successful they will be in developing such interpretations. The probability of developing *appealing* interpretations depends on the quality and quantity of resources devoted to the advocacy effort. In particular, the probability depends on the availability of the requisite resources for the acquisition of information about the consequences of various positions on a proposal *and* the presence of creative, experienced people who can transform information about consequences into interpretations that are appealing to members of Congress. The probability of developing appealing interpretations that are *superior* to those that members presently hold depends upon the advocates devoting more and higher quality resources to the advocacy effort than their opponents.

Advocates can increase the probability of establishing resource advantages over their opponents in two ways. First, they can increase the overall quantity and quality of resources available for advocacy efforts. For example, interest groups and presidents can hire more experienced, creative lobbyists and give them better staff and research support. Similarly, members of Congress can hire more experienced legislative assistants who have established relationships with other topnotch legislative assistants as well as lobbyists of various interest groups. Second, given an existing set of resources, advocates with multiple legislative interests can choose the proposals on which they want to attempt to establish their greatest advantage. On such proposals, presidents and interest groups, for instance, can assign their most experienced, most creative lobbyists and members of Congress can assign their best legislative aides.

However, the extent to which resource advantages translate into legislative influence is conditional upon the length of time available for advocacy efforts. *The shorter the length of time, the smaller the likelihood that any resource*

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[5] A transcript of Magnuson's speech may be found in the *Congressional Record,* Volume 116, Part 16, pp. 22019, 22021-22023.

advantage will have an impact on the decisions of Congress. Advocates must attract both regular and irregular supporters to the coalitions that support the desired positions of the advocates. Advocates rarely have so many regular supporters that they need not worry about the infrequent supporter when building a winning coalition (Smith, 1980, pp. 39-47). Because of the similarity in goals, advocates can quickly develop interpretations that their regular supporters will find attractive and superior to those offered by opposing advocates. However, advocates require far more time to attract the support of irregular supporters. Since there is less similarity in goals, advocates find it more difficult to formulate interpretations that irregular supporters will find attractive, much less superior, to those they may already hold. Moreover, advocates find it difficult to make irregular supporters aware of potentially appealing interpretations. Kingdon (1973, chap. 11), for example, has argued that members pay attention to those actors with whom they historically agree. Advocates will therefore have to work harder (e.g., make personal visits to the member, make repeated presentations of the arguments) to make the member attend to the interpretation. Given sufficient time, advocates who enjoy a resource advantage are likely to experience considerable success in attracting irregular supporters to the supporting coalition. But when time is short, they are likely to have far less success.

5. The extent to which advocates can influence the decisions of the Congress regarding a legislative proposal varies as the proposal is considered in different legislative situations.

This relationship between resource advantage and time creates a significant problem for advocates who seek to pass legislative proposals into law. The problem is that their influence over the congressional treatment of the proposals is likely to vary substantially as the bills move through different legislative situations. Consider two especially significant situations that often arise while a proposal is under consideration on the floors of the House and Senate. In the first situation, members are asked whether their support of an advocate's position on the proposal also extends to support for its position on alternative versions of the proposal, especially those that the advocate views as weakening or destroying the most desirable provisions of the original proposal. These alternative versions are proposed in the form of amendments and recommital motions. In the second situation, members are asked, after having already voted on whether to pass a proposal into law, whether they will take the same position on a conference report, and when applicable, the vote to override the President's veto.

Advocates seeking to pass a proposal into law must maintain their support in both situations; failure to do so can lead to drastic alterations in the proposal, including total defeat. The first situation is especially troublesome. An advocate has a difficult time translating resource advantages into legislative influence. Because the provisions of alternative versions of the proposal are different (often substantially), members have cause to consider whether their interpretations of the proposal also apply to the alternative version, to seek out additional interpretations, and to form new interpretations and positions. The advocate, therefore, needs to present interpretations that result in members supporting its positions on the alternative versions. But time is short; the length of time available for the advocacy effort may range from a few minutes to a few days, but something less than 24 hours is typical. The lack of time creates few difficulties among the regular supporters of the advocate; the basic similarity in goals allows the advocate to devise and disseminate interpretations that regular supporters will find attractive and superior to those offered by opposing advocates. But the lack of time does pose significant difficulties among irregular supporters. Because of the lack of commonality in goals, the advocate finds it difficult and time consuming to develop and get across interpretations that irregular supporters will find more appealing than those offered by the opposing advocates. As a result, regardless of the level of resource advantage, the advocate is especially prone to lose the support of its irregular supporters on alternative versions of a proposal and, consequently, finds it difficult to maintain, much less expand, the size of its supporting coalition in the face of amendments or motions that threaten to weaken or eliminate desirable provisions of the original proposal.

By contrast, in the second situation, an advocate who has established a resource advantage can expect to retain the support of those members who supported the initial passage of the proposal, regardless of whether they are regular or irregular supporters. Moreover, if it desires, the advocate can even expect to gain the support of some of those members who opposed the passage. The reason is the greater length of time available to develop the advocacy effort—time that allows the advocate to make effective use of any resource advantage it may enjoy. The period of time between the dates of the initial passage vote and the conference or veto override votes is typically weeks, often months. An advocate, therefore, has the time to make sure that all members who supported the original proposal still believe their interpretations and positions are applicable to the conference version. For those who are in doubt, the advocate has ample time to use its resource advantage to develop and disseminate interpretations that many of the members will find superior to those offered by opposing advocates. Furthermore, if the advocate chooses, it has enough time to attract the support of some of those members who opposed them on the passage vote. Because of its resource advantage the advocate can create and get across interpretations that some members will view as superior to those they currently hold or those being offered by opposing advocates. The result is that an advocate can expect to maintain and, if it desires, to increase the size of the supporting coalition for the proposal, so long as it can establish a resource advantage.

In sum, the relationships between advocacy, interpretation, and legislative decision making suggest the followng pattern of legislative influence.

An advocate can have substantial success in maintaining and, if he chooses, expanding the size of its supporting coalitions from the time of the passage of a proposal to the time of the conference or veto override vote. Because of the ample time available in this legislative situation, resource advantages are likely to translate into legislative influence: the greater the resource advantage, the greater the success that the advocate is likely to experience in maintaining and increasing the size of its supporting coalitions.

An advocate is likely to have far less success in maintaining the size of its supporting coalition for a proposal in the face of threatening amendments and motions. Because of the lack of time available to develop advocacy efforts, the advocate will find it difficult to use its resource advantage effectively and will, therefore, lose the support of many members, especially irregular supporters. Hence, regardless of the level of its resource advantage, the advocate is likely to experience little success in increasing the size of its supporting coalitions and may often encounter significant difficulties in even maintaining their size.

The influence of an advocate over the decisions of members is, therefore, both substantial and fragile. Despite desires to the contrary, there are legislative situations in which the advocate can experience great difficulty in controlling the outcome. My analyses of the legislative influence of the National Education Association during the 94th Congress offers evidence of this difficulty.

Variations in Legislative Influence: The NEA Experience

Measuring an Advocate's Resource Advantage

Providing evidence of the pattern of influence described above depends critically on measuring the resource advantages of an advocate. However, resource advantages are extremely difficult to measure directly. One major problem is the development of indicators of both the quantity and quality of the resources allocated. Indicators of quality are especially difficult since they must capture such key dimensions as intelligence, creativity, and political savvy. Another major problem is that direct measurement requires the collection of enormous quantities of data. Establishing the level of resource advantage that one advocate enjoys on a legislative proposal involves not only the measurement of that advocate's resource allocation, but also the resource allocations of all other advocates interested in the proposal. This task is extremely demanding: it requires the collection of information that is more systematic and detailed than that found in the most careful and exhaustive legislative case studies to date.

Because of these difficulties, less direct measures of the level of resource advantage are necessary. One such measure is the *priority* given to a legislative proposal by an advocate. It can serve as an excellent indicator of the level of resource advantage for those advocates who are interested in many legislative proposals. The reasons are briefly described below. A more extended discussion may be found in Appendix 1.

Advocates try to influence congressional decisions by developing and executing lobbying strategies. However, advocates face resource constraints that affect both the development and implementation of these strategies. In particular, resources are uneven in quality and limited in quantity. Therefore, advocates must decide how to allocate their resources among several proposals. One method is a priority allocation rule; the more important a legislative proposal, the higher the probability that the necessary quantity and quality of resources are allocated to develop and implement fully the lobbying strategy that the advocate believes is necessary to aid the passage of the proposal. The level of priority indicates the resource advantage established by an advocate so long as there is not a strong positive correlation between the priorities of the advocate and those of opposing advocates.

During the 94th Congress, the lobbyists of the National Education Association used a priority allocation rule to distribute resources among the 60 proposals of interest to their organization. (See Appendix 2 for a list of the proposals.) In the next section, those priorities will serve as the cornerstone of the empirical analyses of the legislative influence of the NEA.

Influencing Conference and Veto Override Decisions

Influencing the conference and veto override votes regarding a legislative proposal is a significant problem for advocates. This was especially the case for the NEA in the 94th Congress. During that time the White House was occupied by a Republican who was unsympathetic to many of the legislative objectives of the NEA. As a result, many of the proposals in Congress that were supported by the NEA encountered the active opposition of the White House, often culminating in a veto by President Ford. In fact, eight legislative proposals that were of interest to the NEA were vetoed by Ford (six were subsequently overridden), with an unknown number of proposals escaping a veto because of the existence of strong levels of support in the House and Senate, both at the time of the passage of the proposal and, more important, at the time of the adoption of the conference report. Thus, in order to be in a position to prevent or override presidential vetoes, an important task for the lobbyists of the NEA was not only to maintain, but to continue to build support for a proposal after it initially passed the House or Senate.

NEA lobbyists are likely to be quite successful in this task. As was argued above, the legislative situation provides ample time to turn resource advantages into legislative influence. As the NEA resource advantage over proposals increases, its lobbyists are more likely to ensure that all members who supported the original proposal at the time of its passage still believe their interpretations and positions are applicable to the conference and veto override versions. They are also more likely to devise and disseminate interpretations that many members who may question the applicability of their original interpretations and positions will find superior to those offered by opposing advocates. Finally, the NEA lobbyists are more likely to create and get across interpretations that some members who opposed the NEA during the passage of the proposal will view as superior to those they currently hold or those being offered by opposing advocates.

For these reasons, a strong, positive relationship is hypothesized to exist between the priority of a proposal and the influence of the NEA on the size of the congressional coalition; that is, the higher the priority given to a legislative proposal by the NEA, the greater the increase in the level of support at each successive consideration of the proposal on the floor of the House or Senate. However, it should be remembered this hypothesized relationship does rest on a key assumption. The NEA lobbyists must have the opportunity to establish a resource advantage when they choose; there cannot be a high positive correlation between the priorities of the NEA and those of opposing advocates. If there were, any gains that the NEA made might be counterbalanced by losses from their irregular supporters.

In order to test this hypothesized relationship between organizational priorities and success in expanding the size of congressional coalitions, the following OLS regression equation was estimated for proposals of interest to the NEA when they were considered in the House of Representatives:

$$\begin{aligned}\Delta\ \text{SUPPORT} &= a + b_1\,\text{PRIORITY} \\ &+ b_2\,\text{INTEREST} + b_3\,\text{MARGIN} \\ &+ b_4\,\text{PRESINT} + b_5\,\text{VETO} + b_6\,\text{TIME} \\ &+ b_7\,\text{APP} + b_8\,\text{EDL} + b_9\,\text{WM} \\ &+ b_{10}\,\text{PW} + b_{11}\,\text{JUD} + b_{12}\,\text{HA} \\ &+ b_{13}\,\text{PO} + e \qquad (1)\end{aligned}$$

where all variables are defined in Table 2 and a is the intercept, the bs are regression coefficients, and e is the error or the disturbance term. The variable of primary interest is PRIORITY. It is expected to have a strong positive coefficient, thereby reflecting the hypothesized ability of advocates to maintain and expand the size of their supporting coalitions. All other variables in equation (1) are included as controls to approximate as nearly as possible *ceteris paribus* conditions. For example, INTEREST is included to control for variations in interest that may translate into variations in influence. Proposals vary in the extent to which they are of interest to advocates. In some cases, such as an education appropriation proposal, NEA lobbyists will be concerned with most, if not all, of the provisions of the proposal. In other cases, their interest may extend to only a few provisions of a proposal, such as the school lunch appropriation section of the Department of Agriculture Appropriations bill. MARGIN is intended to control for those times when the size of the coalition in support of the NEA position was greater than what was needed to pass or even override a potential veto. The relationship between MARGIN and Δ SUPPORT is expected to be negative. Finally, the committee variables (e.g., APP) are included to help control for potential variation in the level of House support that is due to variation in the chambers' reactions to differences in the subject matter and decision making processes of the standing committees (e.g., Fenno, 1973). (The effect on House support for a proposal due to its consideration by the House Budget Committee is included in the intercept.)

Other variables serve as controls, but also have substantive interest. For example, two variables provide evidence of presidential influence in Con-

Table 2. Description of Variables Used to Assess the Legislative Influence of the NEA

Equation	Variable	
1 and 2 only	Δ SUPPORT	The increase in the size of the supporting coalition for a legislative proposal from the time of initial passage of the proposal to the adoption of the conference report and the time of the adoption of the conference report to the vote to override the President's veto. The size of the supporting coalition is measured in terms of the *proportion* of those members who voted for the legislative proposal. All cases in which the level of support on the first vote of the pair exceeded .90 have been excluded from the analyses.
	VETO	Coded 1 if the increase in the size of the supporting coalition concerned a veto override vote; 0 otherwise.
3 and 4 only	Δ SUPPORT	The extent to which the congressional support for a legislative proposal is resistant to amendments or motions which threaten to weaken or kill the proposal. It is computed by subtracting the proportion of members who supported the NEA's position on the legislative proposal from the percentage of members who voted in support of the NEA's position on the weakening amendment or motion to recommit the legislative proposal. A negative difference indicates a loss of support in the face of a threatening amendment or motion.
	CONF	Coded 1 if the weakening action occurred as a motion to recommit the conference report on the legislative proposal, 0 if otherwise.
1, 2, 3, & 4	PRIORITY	The priority that the NEA attached to the legislative proposal. Proposals were given 1 of 15 ordinal rankings, coded on an equal interval scale ranging from 0 to 1. The coefficient, therefore, represents the full range of the NEA's lobbying influence on the proposal of interest to it.
	INTEREST	The extent of NEA's interest in all aspects of the legislative proposal (coded 1 if the NEA is interested in all or most of the provisions of the legislative proposal, 0 if interested in only some of the provisions). The coding is based on a comprehensive review of the publications and internal documents of the NEA.
	MARGIN	The proportion of votes in support of the NEA's position minus the proportion needed to pass the proposal;
	PRESINT	The presidential interest in the legislative proposal (coded 1 if the president takes a position on the legislative proposal, 0 if the president announces no position). The coding is based on information in the *Congressional Quarterly Almanac, 1975* and *1976.*
	TIME	Coded 1 if the legislative proposal was considered during the two periods of peak legislative activity in the 94th Congress, 0 otherwise. Both periods occurred in the second session of the 94th Congress. The first began June 14, 1976 and ended July 1, 1976, the second period from Sept. 15, 1976 to October 1, 1976;
1, 2, 3, or 4	APP	Coded 1 if the legislative proposal originated in the House Appropriations Committee, 0 if otherwise;
	EDL	Coded 1 if the legislative proposal originated in the House Education and Labor Committee, 0 if otherwise;
	WM	Coded 1 if the legislative proposal originated in the House Ways and Means Committee, 0 if otherwise;
	PW	Coded 1 if the legislative proposal originated in the House Public Works and Transportation Committee, 0 if otherwise.
	JUD	Coded 1 if the legislative proposal originated in the House Judiciary Committee, 0 if otherwise;
	PO	Coded 1 if the legislative proposal originated in the House Post Office and Civil Service Committee, 0 if otherwise;
	GO	Coded 1 if the legislative proposal originated in the House Government Operations Committee, 0 if otherwise;

TABLE 2 (continued)

Equation	Variable	
1, 2, 3, or 4	HA	Coded 1 if the legislative proposal originated in the House Administration Committee, 0 if otherwise;
2 or 4 only	SAPP	Coded 1 if the legislative proposal originated in the Senate Appropriations Committee, 0 if otherwise;
	SBUD	Coded 1 if the legislative proposal originated in the Senate Budget Committee, 0 if otherwise;
	SLPW	Coded 1 if the legislative proposal originated in the Senate Labor and Public Welfare Committee, 0 if otherwise;
	SFIN	Coded 1 if the legislative proposal originated in the Senate Finance Committee, 0 if otherwise;
	SPW	Coded 1 if the legislative proposal originated in the Senate Public Works Committee, 0 if otherwise;
	SJUD	Coded 1 if the legislative proposal originated in the Senate Judiciary Committee, 0 if otherwise;
	SPO	Coded 1 if the legislative proposal originated in the Senate Post Office Committee and Civil Service Committee, 0 if otherwise;
	SRUL	Coded 1 if the legislative proposal originated in the Senate Rules and Administration Committee, 0 if otherwise.

Table 3. Influencing Conference and Veto Override Decisions: The NEA Experience in the House of Representatives–94th Congress (N = 24)[a]

			Regression Coefficients	Standardized Regression Coefficients	t value[b]
Δ SUPPORT =	a		−.062	–	–
	b_1	PRIORITY	.085	.32	1.86
	b_2	INTEREST	.081	.50	2.25
	b_3	MARGIN	−.531	−.86	4.12
	b_4	PRESINT	−.046	−.29	1.42
	b_5	VETO	−.094	−.57	3.16
	b_6	TIME	.058	.34	2.98
	b_7	APP	.145	.85	4.82
	b_8	EDL	.252	1.07	6.34
	b_9	WM	.117	.42	3.04
	b_{10}	PW	.184	.88	4.35
	b_{11}	JUD	.152	.39	2.61
	b_{12}	HA	.206	.53	3.04
	b_{13}	PO	.046	.16	.79

(The House Budget Committee is included in the Constant)

$R^2 = .93; \bar{R}^2 = .83; S.E. = .033$

[a] This analysis does not include eight cases in which the level of support expressed on the first vote of the pair exceeded 90%. When included, the regression coefficients of prime interest (e.g., PRIORITY) remain largely unchanged, but the total variance explained by the equation declines considerably ($\bar{R}^2 = .69$).

[b] Given the degrees of freedom, a t value of at least 1.37 is necessary for a .10 level of significance, 1.81 for a .05 level of significance. However, since the entire population of cases is under analysis, the meaning of the t values is ambiguous. They are included for the interested reader.

gress. PRESINT controls for influence that is due to the White House simply announcing a position on the legislative proposal. Since the NEA was frequently at odds with the White House during the 94th Congress, the variable is expected to have a negative coefficient. VETO controls for the possibility that it may be more difficult to expand the size of a supporting coalition to override a presidential veto than to approve a conference report (and perhaps stave off a veto). The variable, therefore, is also expected to have a negative coefficient. TIME is another control variable of substantive interest. It was included in equation (1) to indicate if the consideration of the conference report or the veto override occurred during a period of peak legislative activity. During such periods, members are more likely to rely on cue taking since they have less time to consider personally any proposals outside their committee jurisdictions. If lobbyists can persuade some of the cue givers who were previously hostile or who took no position on the proposal to support it, then many more members are also likely to support the proposal than would be the case if cue taking was at lower levels. TIME is, therefore, expected to have a positive coefficient.

Table 3 presents the results of OLS estimation of equation (1). As expected, the PRIORITY coefficient is positive. Given two legislative proposals, one given the NEA's highest priority and the other the lowest, the NEA lobbyists were able to expand the size of the supporting coalition for the high priority proposal by about 9% of the members who were present and voting (typically 30 to 35 House members). This is the case whether the lobbyists are trying to increase the level of support from the time of passage or from the time of the adoption of the conference report. However, attempts to expand the supporting coalition that existed at the time of the conference report (in preparation for overriding a presidential veto) are generally undertaken under less favorable circumstances than is the case at the time of passage. In fact, unless the legislative proposal receives the highest priority of the NEA, the supporting coalition is likely to *decrease* in size. The VETO coefficient indicates that in the face of a Ford veto, approximately 9% of the House membership who originally supported the conference report version of the proposal will no longer do so.

The values of the other coefficients in the equation seem quite reasonable. Both the coefficients of MARGIN and PRESINT, for example, have the predicted, negative sign. Also, the TIME coefficient is positive as was hypothesized; that is, whenever there is a period of peak legislative activity, the task of expanding the supporting coalition is considerably easier. Taken together, then, the estimates of the coefficients in Table 3 appear quite plausible.

As for the Senate, there were too few cases to conduct a separate analysis. The only feasible alternative was to do a combined estimation of the House and Senate cases. The following equation was estimated.

$$\begin{aligned}\Delta\ \text{SUPPORT} = a &+ b_1\,\text{PRIORITY} \\ &+ b_2\,\text{INTEREST} + b_3\,\text{MARGIN} \\ &+ b_4\,\text{PRESINT} + b_5\,\text{VETO} + b_6\,\text{TIME} \\ &+ b_7\,\text{APP} + b_8\,\text{EDL} + b_9\,\text{WM} + b_{10}\,\text{PW} \\ &+ b_{11}\,\text{JUD} + b_{12}\,\text{HA} + b_{13}\,\text{PO} + b_{14}\,\text{SAPP} \\ &+ b_{15}\,\text{SBUD} + b_{16}\,\text{SLPW} + b_{17}\,\text{SFIN} \\ &+ b_{18}\,\text{SPW} + b_{19}\,\text{SPO} + b_{20}\,\text{SRUL} + e \qquad (2)\end{aligned}$$

where all variables are defined in Table 2.

The results of the OLS estimation are presented in Table 4. The coefficient estimates continue to offer support for the hypothesized relationships. The addition of the thirteen cases from the Senate did not alter the basic relationships among the variables of equation; the values of the coefficients of all the variables are remarkably similar to those in Table 3. Moreover, in preliminary estimations of equation (2), a variety of interaction terms were included to control for Senate specific differences in the effects of variables (like PRIORITY) on Δ SUPPORT. The addition of those terms did not alter the value of the PRIORITY coefficient. Nor did their addition increase the level of explained variance significantly. Hence, by several indicators, the addition of the Senate cases to the analyses suggests that the NEA's lobbying influence was no different in the Senate from in the House.

Maintaining Support in the Face of Threatening Motions

If advocates are to have a full measure of control over the destiny of the legislative proposals of interest to them, they need to be concerned about more than influencing the size of the congressional coalitions that support them on conference and veto override votes. They also need to be concerned about maintaining the size of their supporting coalitions in the face of motions or amendments that threaten to weaken or eliminate those aspects of the committee proposal that the advocate favors. The appearance of weakening amendments and recommital motions represent the major opportunities for the opponents of a committee-approved legislative proposal to test the resolve of supporting members and, hence, the stability of supporting coalition.

However, advocates, including the NEA, are not likely to be able to do much to maintain the size of their supporting coalitions. As was argued

Table 4. Influencing Conference and Veto Override Decisions: The NEA Experience in Both the House and Senate–94th Congress (N = 37)[a]

			Regression Coefficients	Standardized Regression Coefficients	t value[b]
Δ SUPPORT =	a		–.066	–	–
	b_1	PRIORITY	.085	.39	1.39
	b_2	INTEREST	.071	.52	1.49
	b_3	MARGIN	–.443	–.78	2.68
	b_4	PRESINT	–.049	–.36	1.13
	b_5	VETO	–.082	–.55	2.22
	b_6	TIME	.051	.32	2.23
	b_7	APP	.134	.78	3.25
	b_8	EDL	.239	.96	4.38
	b_9	WM	.114	.38	2.18
	b_{10}	PW	.166	.76	2.89
	b_{11}	JUD	.138	.33	1.71
	b_{12}	HA	.211	.50	2.31
	b_{13}	PO	.051	.17	.65
	b_{14}	SAPP	.133	.44	2.35
	b_{15}	SBUD	.094	.38	2.11
	b_{16}	SLPW	.172	.41	2.48
	b_{17}	SFIN	.114	.46	1.69
	b_{18}	SPW	.182	.61	3.14
	b_{19}	SPO	.036	.07	.34
	b_{20}	SRUL	.080	.19	.89

(The House Budget Committee is included in the constant)

$R^2 = .81; \bar{R}^2 = .56; S.E. = .045$

[a] This analysis does not include 13 cases in which the level of support expressed on the first vote of the pair exceeded 90%. When included, the regression coefficients of prime interest (e.g., PRIORITY) remain largely unchanged, and the total variance explained by the equation declines only slightly ($\bar{R}^2 = .53$).

[b] Given the degrees of freedom, a t value of at least 1.34 is necessary for a .10 level of significance, 1.75 for a .05 level of significance. However, since the entire population of cases is under analysis, the meaning of the t values is ambiguous. They are included for the interested reader.

above, the proposal of amendments and recommital motions creates a legislative situation in which it is difficult to turn resource advantages into legislative influence. Because the provisions of alternative versions of the proposals are different (often substantially), members have cause to seek out and develop new interpretations and positions. But there is little time to develop and to disseminate the interpretations that will result in members supporting the advocate's position on a legislative proposal. An advocate is especially prone to lose its irregular supporters since they are most likely to become aware of and find persuasive those interpretations offered by opposing advocates. Consequently, the lobbyists of the NEA are likely to experience little success in maintaining supporting coalitions in the face of threatening amendments and motions. There is likely to be little relationship between the priority given a legislative proposal and the stability of a supporting coalition.

To test this hypothesis, the following OLS regression equation was estimated for proposals of interest to the NEA when they were considered in the House of Representatives.

$$\begin{aligned}\Delta\ \text{SUPPORT} = {} & a + b_1\,\text{PRIORITY} \\ & + b_2\,\text{INTEREST} + b_3\,\text{MARGIN} \\ & + b_4\,\text{PRESINT} + b_5\,\text{TIME} + b_6\,\text{CONF} \\ & + b_7\,\text{APP} + b_8\,\text{EDL} + b_9\,\text{WM} + b_{10}\,\text{GO} \\ & + b_{11}\,\text{PW} + b_{12}\,\text{JUD} + b_{13}\,\text{PO} + e. \qquad (3)\end{aligned}$$

where all variables are defined in Table 2 and a is the intercept, the bs are regression coefficients, and e is the error or the disturbance term.

Once again, the variable of primary interest is PRIORITY. This time, however, the coefficient is expected to be near zero. As before, all other variables in the equation serve as controls. The rationale for most of them has been given above, but a few words of explanation are in order about three of the variables. First, a new variable, CONF, was included in order to control for any differences in the House response to recommittal motions concerning conference reports compared to the response to weakening amendments and recommittal motions offered during the initial consideration of the proposal. Second, as was the case in the previous legislative situation, MARGIN was included to control for those times when the NEA enjoyed a large margin of support and, therefore, did not have to be so concerned about the stability of that support. MARGIN is expected to have a negative coefficient. Finally, TIME was included in the equation to indicate those periods in which the House's consideration of the proposal and any weakening amendments or motions was likely to be rushed. Hasty consideration of a proposal is likely to decrease the stability of the supporting coalition. During the periods of peak legislative activity, there is likely to be a greater reliance on cue taking. Hence, the defection of cue givers, especially those who are not regular supporters of the NEA's positions, is likely to affect a greater number of members than during more slack periods. Accordingly, the TIME coefficient is expected to be negative.

The results of the OLS estimation for equation (3) are presented in Table 5. They are consistent with the hypothesis. The PRIORITY coefficient is small, of the wrong sign, and statistically insignificant, thereby suggesting the absence of a relationship between NEA's legislative priorities and the stability of its supporting coalitions on all proposals of interest to it. This finding seems especially plausible in light of the reasonable signs of the coefficients of other noteworthy variables. For example, the TIME coefficient has a negative sign (as expected), suggesting that the crush of legislative business can make it even more difficult for the NEA lobbyists to hold on to the House support expressed for the NEA's positions on the proposals of interest to it. Moreover, as was the case in the first legislative situation, the task for the NEA's lobbyists was made even more difficult when the Ford administration took an official position on the proposal and the weakening amendment or motion. That increased difficulty is reflected in the negative sign of the PRESINT coefficient. And as a final example, the negative sign of the MARGIN coefficient indicates that the level of instability of the supporting coalition is indeed related to the level of excess support

Table 5. Maintaining Support for Proposals in the Face of Threatening Motions: The NEA Experience in the House of Representatives–94th Congress (N = 21)

			Regression Coefficients	Standardized Regression Coefficients	t value[a]
Δ SUPPORT =	a		.131	–	–
	b_1	PRIORITY	–.041	–.10	.64
	b_2	INTEREST	.002	.01	.03
	b_3	MARGIN	–.271	–.35	2.05
	b_4	PRESINT	–.091	–.26	2.41
	b_5	TIME	–.161	–.63	4.32
	b_6	CONF	–.123	–.52	4.55
	b_7	APP	–.044	–.21	1.18
	b_8	EDL	–.170	–.59	3.67
	b_9	WM	–.131	–.38	1.76
	b_{10}	GO	.003	.01	.05
	b_{11}	PW	.070	.20	1.20
	b_{12}	JUD	–.190	–.40	2.44
	b_{13}	PO	–.140	–.29	1.46

(The House Budget Committee is included in the constant)

$R^2 = .96; \bar{R}^2 = .90; S.E. = .033$

[a] Given the degrees of freedom, a t value of at least 1.42 is necessary for a .10 level of significance, 1.90 for a .05 level of significance. However, since the entire population of cases is under analysis, the meaning of the t values is ambiguous. They are included for the interested reader.

Table 6. Maintaining Support for Proposals in the Face of Threatening Motions: The NEA Experience in Both the House and Senate–94th Congress ($N = 29$)

			Regression Coefficients	Standardized Regression Coefficients	t value[a]
Δ SUPPORT =	a		.117	–	–
	b_1	PRIORITY	–.019	–.05	.23
	b_2	INTEREST	.050	.23	.83
	b_3	MARGIN	–.373	–.42	2.65
	b_4	PRESINT	–.008	–.02	.21
	b_5	TIME	–.094	–.33	2.47
	b_6	CONF	–.099	–.35	2.77
	b_7	APP	–.049	–.20	1.02
	b_8	EDL	–.176	–.50	2.93
	b_9	WM	–.168	–.40	1.88
	b_{10}	GO	–.030	–.05	.36
	b_{11}	PW	.003	.01	.04
	b_{12}	JUD	–.199	–.34	1.99
	b_{13}	PO	–.156	–.27	1.31
	b_{14}	SAPP	–.088	–.21	1.55
	b_{15}	SBUD	–.006	–.01	.12
	b_{16}	SFIN	–.163	–.39	1.60
	b_{17}	SJUD	–.373	–.64	3.73
	b_{18}	SPO	–.053	–.09	.43

(The House Budget Committee is included in the constant)

$R^2 = .94; \bar{R}^2 = .83; S.E. = .045$

[a] Given the degrees of freedom, a t-value of at least 1.37 is necessary for a .10 level of significance, 1.81 for a .05 level of significance. However, since the entire population of cases is under analysis, the meaning of the t values is ambiguous. They are included for the interested reader.

expressed for the legislative proposal. Taken together, then, the results presented in Table 5 appear to be plausible estimates that are most consistent with the hypothesis of little or no legislative influence.

As for the influence of the NEA lobbyists in the Senate, there are no apparent institutionally based reasons to expect that their influence on the stability of Senate supporting coalitions would differ from what it is in the House. In fact, the absence of a relationship between an organization's lobbying priorities and the stability of the supporting coalition seems even more plausible, given the greater unpredictability of the Senate treatment of committee-approved legislative proposals (e.g., Fenno, 1973). Unfortunately, there were too few cases to conduct a separate analysis of Senate decisions. Once again a combined estimation of the decisions of the House and Senate was the only alternative. The following equation was estimated.

$$\begin{aligned}\Delta\ \text{SUPPORT} = {} & a + b_1\,\text{PRIORITY} + b_2\,\text{INTEREST} + b_3\,\text{MARGIN} \\ & + b_4\,\text{PRESINT} + b_5\,\text{TIME} + b_6\,\text{CONF} \\ & + b_7\,\text{APP} + b_8\,\text{EDL} + b_9\,\text{WM} + b_{10}\,\text{GO} \\ & + b_{11}\,\text{PW} + b_{12}\,\text{JUD} + b_{13}\,\text{PO} + b_{14}\,\text{SAPP} \\ & + b_{15}\,\text{SBUD} + b_{16}\,\text{SFIN} + b_{17}\,\text{SJUD} \\ & + b_{18}\,\text{SPO} + e \qquad (4)\end{aligned}$$

where all the variables are defined as before.

The results of the estimation are presented in Table 6. As before, they indicate no relationship between the NEA's legislative priorities and the stability of its support. Moreover, the other coefficients of interest retain the same sign with the exception of the PRESINT coefficient which declined to zero, suggesting that presidential influence in the Senate differs from the House. Also, a variety of interaction terms were included in preliminary estimations to control for Senate specific differences in the effects of the independent variables (like PRIORITY) on Δ SUPPORT. In no case did the addition of such terms change the value of the PRIORITY coefficient. Nor did their addition change the explained variance appreciably. By several indicators, then, the addition of the Senate cases to the analyses does not produce different estimates. In both the House and the Senate, the lobbyists of the NEA appear to be unable to influence the stability of their supporting coalitions.

Conclusions

One of the primary aims of the scholarly study of governmental institutions is to develop theories about policy formation. This research has sought to establish interpretation as a significant variable in such theories. In so doing, the research has indicated that the level of congressional support for a policy alternative is fragile. Those who seek to formulate and shape public policy may experience considerable success, but their success may be fleeting.

This conclusion (and the arguments and evidence supporting it) have implications for at least four streams of research regarding congressional policy formation. First, they underscore the existence of short-term change in the voting decisions of members of Congress—change that is seen as critical by advocates trying to build winning coalitions. This emphasis on short-term change is in contrast to the existing models of legislative voting which have stressed the continuity and stability of members' voting decisions. (For reviews and recent work, see Asher & Weisberg, 1978; Poole, 1981; Poole & Daniels, 1982.) There is no question that considerable continuity and stability exist. Indeed, advocates count on the continued support from their regular supporters in their efforts to influence congressional decisions. At the same time, if advocates are to develop winning coalitions, they must induce some members to take positions that are not consistent with their historical positions. This article suggests how and why advocates are partially successful in these efforts. More complete models of legislative voting need to emphasize both the stable and dynamic elements of voting (see Weisberg, 1978, for one attempt).

Second, this research is further evidence against those interest-group dominance theses that continue to have currency today, especially in journalistic and "economic" explanations of legislative decision making (e.g., Mayhew, 1975). The NEA is an impressive organization. It has a well-run, high quality lobbying operation, supported by extensive research, political action, and public relations efforts. It also has advantages that many interest groups do not have, primarily in the form of a large (approximately 1.8 million members), well-educated, politically active membership which is fairly evenly distributed across most congressional districts. Clearly, when coupled with well-planned lobbying campaigns these advantages have an effect on Congress. The NEA was found to have considerable influence in increasing the size of supporting coalitions, but it would be a mistake to interpret this finding as evidence of interest-group dominance since, in an important sense, the NEA cannot control its destiny. Try as it might, the NEA seems to be unable to exert influence on the stability of supporting coalitions. Their ability to protect key provisions of a legislative proposal is, therefore, limited. If this result is found to generalize to other interest groups (and there is no reason to believe the NEA is a special case), then an important limitation on interest group "power" has been identified.

Third, this research suggests that the sources of influence of interest groups (and more generally advocates) are more complex than typically portrayed. Students of lobbying have long argued that the major effect of lobbying is to increase the intensity of the commitment of members of Congress to an already established position on a legislative proposal. Lobbyists are seen as reinforcing and mobilizing those members who already support the organization's position on the proposal. (For examples, see Bauer, Pool, & Dexter, 1972, Part IV; Berry, 1977, pp. 216-223; Kingdon, 1973, chap. 5; and Milbrath, 1963.) Previous work, however, has paid little attention to interpretations. As a result, the influence of lobbyists on establishing and altering (rather than just reinforcing) the positions of members of Congress has been underemphasized. By recognizing the significance of interpretations, a whole new process of influence becomes clear, one that suggests that the influence of lobbyists is both greater and more limited than commonly described in much of the academic literature.

Finally, the emphasis on interpretation in this research stands in contrast to the more familiar models of coalition building that emphasize exchange. In these models, advocates receive the support of members of Congress on a legislative proposal because they can offer attractive exchanges, for example, the promise and delivery of electoral resources, the provision of assistance in securing an important public works project or key committee assignment, or the promise of support on some other bill of strong interest to the member. Exchange models have a well-deserved and important role in explaining influence. But a major implication of this study is that exchange models are not the only, or even the most powerful, models of influence.

This article presents evidence of a pattern of influence that is difficult to explain by an exchange model. The inability of the NEA to control defections among their supporters in the face of threatening amendments is especially significant. For even if one assumes that the exchanges are conditional upon future exchanges (that is, an advocate can count on a member's support so long as a better deal does not come along), it is still possible for advocates to control to some extent when defections occur. For example, the lobbyists of the NEA could make their best deals on legislative proposals of highest priority to the Association. They would make it clear that the NEA's electoral support (or opposition) for members rested on their support of the NEA positions on those proposals. Defection from that position on key threatening amendments would cost the members the electoral support of the Association (or engender its opposition). And because it concentrated its resources on the highest priority proposals, the NEA would be in a poor position to offer attractive exchanges on legislative proposals of moderate to low priority. Therefore, one would expect the NEA would be most successful at preventing defections on high priority proposals.

Since the evidence is inconsistent with this expectation, it casts considerable doubt on the veracity of an exchange explanation. Of course, one might conclude that the NEA's lack of success in preventing defections is simply evidence of an overall lack of influence. But then one would have to explain its apparent influence on conference and veto override decisions. In short, to understand coalition building, a richer set of influence processes needs to be considered than simply those emphasizing exchange. The attempt of this article to lay out the significance of the interpretation process is one step toward this end.

Of course, much work still needs to be done to develop and refine my arguments and findings. At a minimum, the work should be extended to other interest groups, other types of advocates, other stages of the congressional process (notably committee and subcommittee decision making), and other legislatures. In addition, the role of interpretations in decision making and influence needs to be better established. Some progress is possible in natural settings. One promising strategy is to arrange frequent, regularly scheduled interviews with representatives and their staff assistants in order to record their interpretations of legislative proposals as they develop and move through the legislative process. The major drawback of this strategy is that it requires the availability, patience, and good will of representatives. Since all appear to be in increasingly short supply to students of the U.S. Congress, new research may be most feasible at the state and local levels. At the same time, some questions about interpretations, especially the conditions under which their stability may vary, might also be pursued through experimentation in controlled laboratory settings. These experimental findings could both supplement and inform investigations in natural settings. Whatever the research strategy, the result of this additional work will further the goal of developing theories of policy formation—theories that will lead to a better understanding of the workings of government and the design of reforms to change it.

Appendix 1: The Priority Measure

In order to understand more clearly the reasons why a priority allocation rule is a good measure of the resource advantages of organizations like the NEA, the concepts of lobbying strategy, scarce resources, and priority allocation require some additional comment. Lobbying strategies are collections of lobbying activities that an organization's lobbyists believe are all the organization can do to aid the passage of a legislative proposal. Lobbying strategies consist of a wide range of lobbying activities that are combined and weighted in kaleidoscopic variety. The activities are well-known. For example, they include the making of presentations of legislative arguments (in the form of testimony, letters, and telegrams or personal presentations) and the organization and coordination of a variety of grassroots activities, such as letter and telegram campaigns and constituents' visits to members.

The content of lobbying strategies can vary across legislative proposals. The variations are, in part, a reflection of the perceptions of an organization's lobbyists of the difficulties involved in the passage of the legislative proposal. Their perceptions are based on such factors as their knowledge of which members support or oppose the proposal and with what intensity, the level of organizational and constituent interest in the legislation, and the commitment from the White House. As the perceived level of difficulty of passing a legislative proposal increases, the lobbying strategy is likely to be adjusted correspondingly. For example, if there is a great deal of interest and well-defined opposition to a legislative proposal both in the Congress and among lobbying organizations, an organization's lobbyists may believe that an intensive and extensive lobbying effort is required if they are to help pass the proposal. In the case of the NEA, an intense and extensive lobbying effort means that the lobbying staff would try to make presentations to large numbers of members of Congress both by letter and in person. The lobbyists would also encourage their extensive activist network of members to produce letters and telegrams. And if there were time, the lobbyists might coordinate and encourage the NEA membership to contact their members either at home or in Washington. By contrast, a non-controversial proposal requires a less extensive lobbying effort. The lobbying strategy for the NEA might consist of the lobbyists making presentations to members of the subcommittee with jurisdiction over the proposal.[6]

The development of an effective lobbying strategy is dependent on having accurate knowledge of the potential congressional action on the proposal. Intelligence gathering is, therefore, a major, unremitting activity of effective lobbyists, who attempt to discern the nature of the congressional support for the proposal and the kind of actions, especially threatening ones, that are being planned by members of Congress, perhaps on behalf of or in cooperation with their staffers or other organizational lobbyists. For instance, lobbyists seek information about which members are going to offer amendments or circulate "Dear Colleague" letters.

Both the development and implementation of effective lobbying strategies is critically dependent on the availability of the necessary organizational resources. However, organizations typically lack both the quantity and quality of resources required to develop and implement fully lobbying strategies for all the proposals of interest. Organizations have only a finite amount of staff hours, research support, money, and other organizational resources to devote to lobbying, and it is often a quantity that falls short of what is needed to lobby many proposals (see Bauer, Pool, & Dexter, 1972, chap. 23; and Berry, 1977, for an extended discussion). Moreover, those resources that are made available are not comparable in quality. Most important, some of the lobbyists on a government relations staff are likely to be more experienced and creative than their colleagues, thereby increasing the likelihood that they will be more successful in performing certain lobbying activities. More effective lobbyists have far greater knowledge of the procedural rules of the House and Senate, which can be significant in structuring lobbying strategies; they have the more extensive contact network and greater political savvy required for superior intelligence gathering and more of the personal qualities and experience in "reading" members of Congress necessary for making successful presentations of interpretations.

Because there are constraints on both the quantity and quality of the resources made available for lobbying activities, the problem for an organization's lobbyists is to decide how to allocate those resources. During the 94th Congress, the lobbying efforts of the NEA appeared to be guided by a priority allocation rule. In interviews conducted in August of 1977, NEA lobbyists had no difficulty in specifying in detail the priority they had given to different legislative proposals. Moreover, over several months of observation it became clear that the less important legislative proposals were lobbied by the less experienced and less able members of the NEA's staff. Furthermore, the willingness to devote the resources required to develop and implement a lobbying strategy declined as the priority attached to the legislative proposals declined. The lower willingness ranged from not making full use of the NEA's extensive grassroots network when needed to not assigning the full complement of lobbyists required to canvas the Congress on a potentially close vote.

Finally, the priorities seem to be largely internally determined; they seem to reflect only gross features of the legislative realities in ways that do not override the organization's internal statements of priorities. Contrary to what one might expect, the NEA's legislative priorities do not reflect the lobbyists' beliefs about where they are likely to be successful; rather, they reflect the lobbyists' commitment to where they want to be effective. The best available evidence for this point is reported in Table A1, where the priorities of legislative proposals of interest to the NEA and

Table A1. The Legislative Priorities of the NEA and the Incidence of Congressional Floor Action on Legislative Proposals (N = 44)[a]

	Legislative priority			
Congressional action	High	Moderate	Low	Total
Floor action	8	10	14	32
No floor action	3	3	6	12
Total	11	13	20	44

[a] Appropriation and budget proposals are excluded from the analysis since all such proposals were routinely placed on the congressional agenda. See Appendix 2 for a list of the proposals.

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[6] Variations in the content of lobbying strategies are also due to the organization's lobbyists' perceptions of the competition from other lobbying organizations. If the lobbyists of an organization are supportive of a legislative proposal whose passage is already being lobbied extensively by another organization, their lobbying strategy may consist of maintaining only a nominal presence. For more on this point, see Berry (1977), p. 205.

on which the Congress took some floor action are compared to those proposals on which no floor action was taken in either house. Clearly, the NEA's legislative priorities are not sensitive to extreme variations in the probability of successful passage. Legislative proposals that did not receive active consideration by the Congress had the same priority distribution as those which did. Consequently, smaller variations in the probability of successful passage, are likely to have only a small, perhaps negligible, effect on the legislative priorities. The legislative priorities of the NEA appear to be a good measure of what its lobbyists wanted to accomplish in the Ninety-fourth Congress.

Appendix 2: The Legislative Interests of the NEA

Listed below are the legislative interests of the National Education Association during the Ninety-fourth Congress. The list was compiled from a variety of NEA publications and internal documents and was verified by NEA lobbyists. An asterisk indicates when the Congress took recorded floor actions on the proposal, thereby making it eligible for inclusion in the data sets used to examine the legislative influence of the NEA.

Appropriations

FY 1976 Education Appropriations (HR 5901)*
Emergency Employment Appropriations (HR 4481)*
FY 1975 Supplemental Appropriations (HR 5899)*
FY 1976 First Supplemental Appropriations (HR 10642)*
FY 1976 Agricultural Department Appropriations (School Lunch) (HR 8561)*
FY 1976 Interior Department Appropriations (Indian Education) (HR 8773)*
FY 1976 Regular Labor-HEW Appropriations (HR 6069)*
FY 1976 Second Supplemental Appropriations (HR 13172)*
FY 1977 Labor-HEW Appropriations (HR 14232)*
FY 1977 Agriculture Department Appropriations (School Lunch) (HR 14237)*
FY 1977 Interior Department Appropriations (HR 14231)*
FY 1977 Public Works Employment Appropriations (HR 15194)*

Budget Control

First Concurrent Budget Resolution—FY 1976 (H Con. Res. 215 and S. Con. Res. 32)*
Second Concurrent Budget Resolution—FY 1976 (H Con. Res. 466 and S. Con. Res. 76)*
First Concurrent Budget Resolution—FY 1977 (H Con. Res. 611 and S. Con. Res. 109)*
Second Concurrent Budget Resolution—FY 1977 (H Con. Res. 728 and S. Con. Res. 139)

Authorizations

General Revenue Sharing Extension (HR 13367)*
Higher Education Act Amendments (HR 12851 and S 2546)*
Higher Education Guaranteed Student Loans (HR 14070)*
Vocational Education Act Amendments (HR 12835)*
Cabinet-Level Department of Education (HR 9010, HR 11424, HR 9122)
Juvenile Delinquency in the Schools Act of 1975 (HR 9662 and S 1440)
Gun Control (HR 11196)
Elementary and Secondary Assistance (HR 10145 and S 2546)
Impact Aid Amendments (HR 5181)
Child Nutrition Programs (HR 4222 and S 850)*
Emergency Summer Child Nutrition Programs (S 1310)*
Day Care Standards (HR 9803)*
Day Care Assistance (HR 12455)*
Reading Motivation—Book Distribution (HR 8304)
Metric Conversion (HR 8674)
Young Adult Conservation Corps (HR 10168)*
Public Works (HR 5247)*
Public Works Employment (HR 12972 and S 3201)*
Public Service Jobs (HR 11453)*
Public Service Jobs (HR 12987)*
Quality Education—Emergency Financial Assistance (HR 11668)
Block Grants (HR 12196)
Education of Handicapped Children (S 6)*
White House Conference on Handicapped (SJ Res 154)
Indochina Refugee Education (HR 7897 and S 2145)*
Indochina Refugee Education (HR 8949)
Collective Bargaining—Amendment of National Labor Relations Act (HR 77)
Mobile Teacher Retirement (HR 808 and S 297)
Unemployment Compensation for Teachers (HR 10210)*
Health Security Act (HR 21 and S 6)*
Comprehensive School Health Education Act (HR 2599)*
Voting Rights Extension (HR 6219)*
Overseas Voting Rights (S 95)*
Voter Registration (HR 11552)*

Federal Employees Political Activities—Hatch Act Revision (HR 8617)*
Federal Election Commission Reconstitution (S 3065)*
Copyright Revision of 1909 Law (S 22)*
Public Broadcasting (HR 6461)*
Educational Broadcasting (HR 9660)
National Women's Conference (HR 9924)
Library Construction Facilities (HR 11266)
Daylight Savings Time (HR 13089 and S 2931)
Economic Development Act (S 2228)*
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Interest Representation: The Dominance of Institutions

ROBERT H. SALISBURY
Washington University

Interest group theory traditionally assumed that policies advocated by group representatives in some sense grow out of the interests or values of the group's members. Mancur Olson and others compelled important revisions in this assumption, but still left the process of interest advocacy to membership groups. It is contended here that institutions, such as corporations or local governments, occupy a dominant position with respect to interest representation in Washington, and this finding requires substantial revisions in both theoretical and descriptive formulations of the governmental process.

It is common for academics to divide the American political world into two parts, governmental and nongovernmental. In turn, the actors occupying nongovernmental roles of significance sufficient to require textbook chapters treating them are individual citizens (also aggregated into "public opinion"), political parties, and interest groups. The latter two were once often closely tied together, in texts and in courses, with parties clearly the dominant concern (e.g., Key, 1964). In recent years interest groups have come to constitute a largely autonomous subject matter, but almost always they are placed into a conceptual framework that links them closely to parties. Functional arguments assign groups and parties to the articulation-aggregation portions of the functional space, and process formulations place both on the input side of policymaking. By treating parties and interest groups as two types of political organization, Wilson (1973) has perhaps best reflected the implicit conceptual assumption governing descriptive work and shaping the definitions of theoretical problems.

The American political universe, in fact, contains a considerably more diverse array of actors than these conventional headings suggest. In particular, notable omissions from textbook treatments and most research literature include individual corporations, state and local governments, universities, think tanks, and most other *institutions* of the private sector.[1] Likewise unnoticed are the multitudes of Washington representatives, freestanding and for hire, including lawyers, public relations firms, and diverse other counsellors. Ad hoc coalitions, issue networks, and other "loosely coupled" cooperative structures of activity are occasionally acknowledged, but are rarely described or given a place in academic characterizations of the essential features of the American system. Yet, attentive readers of what might be called the "inside press" of American politics, such as *Congressional Quarterly* and *National Journal,* are regularly informed about these kinds of activity and given to understand that they are often of critical importance to policy outcomes.

In this article I propose to argue that institutions have come to dominate the processes of interest representation in American national politics. Institutions present somewhat different theoretical problems from those we are accustomed to encountering in regard to membership-based interest groups. Moreover there are important empirical differences between a system driven by membership groups and one in which institutions occupy center stage. As the characterization of Washington representation undergoes reconstruction, two important strands of empirical theory

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[1] Business corporations are sometimes considered, sensibly enough, among business interests, but the profound differences in organizational structure and motivation between, for example, General Motors and the Chamber of Commerce have not been remarked, nor has the implication these differences have for interest group theory.

come under scrutiny, interest group theory and theories of representation. It is thus an enormously ambitious undertaking, even presumptuous, that is attempted here. It is intended really as a beginning, however, or perhaps a midstream rechanneling, so as better to accommodate the observables of Washington politics.[2]

There are four essential concepts involved in the proposed reconstruction: interest, organized interest group, institution, and representative. I will seek where appropriate to incorporate the results of recent discussions of these ideas, and in the latter part of the article I will present some fragmentary data illustrating and amplifying the arguments. In general, I will not be concerned as much with defining the key terms with unchallengable clarity as with conveying rather more ostensively the meanings and uses I believe important. In any case, the words bear such heavy accumulations from centuries of everyday employment that it would be of little help or effect to legislate. And, of course, there remains Arthur Bentley's rather insouciant sentiment, "Who likes may snip verbal definitions in his old age, when his world has gone crackly and dry" (1908, p. 199).

The Concept of Interest

Toward what end is political activity directed? At one level we might speak of justice, equity, or the redress of grievances. In a more neutral vein, a number of terms are employed, often more or less interchangeably; e.g., values, objectives, and interests. But it is interest that is most often incorporated into our skeins of theory and thus merits special attention. Interest refers to attitudes, of course (Truman, 1951; cf. MacIver, 1932). It involves values and preferences. But it is the perceived or anticipated effects of policy—government action or inaction including all its symbolic forms as well as more tangible allocations—upon values that create politically relevant interests. Similarly, interested behavior expresses policy-related purpose, sometimes very broadly defined and sometimes highly specific and detailed. To be sure, preferences may be inert, quiescent, held *in pectore,* or otherwise unattached to any visible action. Yet it is often risky for an outside observer to impute interests, asserting that a given policy *ought* to be seen as having such-and-such an effect on certain values, thus producing interests and, perhaps, eventual political action. Finally, interests are certainly perceived by individuals, one at a time and sometimes uniquely, but it is also possible for other reasonably "unitary" actors to perceive and act politically upon interests. Here we come to the institutional actor, and in examining institutions and their interests we must consider how, if at all, they differ from more conventional notions of organized interest groups.

How Interests Are Organized into Groups

Until the publication of Mancur Olson's *The Logic of Collective Action* (1965), comparatively little serious attention had been given to the question of how and whether interests would become organized into associations capable of politically interesting action. Some commentators (e.g., Hagan, 1958), following Bentley's lead, refused to make a distinction between interest and group, treating them as identical phenomena, two words for the same observable activity. In this tradition the main intellectual task was one of mapping, locating the actors involved in the political situation and specifying the policy direction they took. Once all the interests were thus plotted, one would have a complete picture (Bentley, 1908, p. 269), and such a picture, it seems, was expected to yield satisfactory understanding. In any case, Bentley was so anxious not to step outside a strict empirical frame of analysis that little attention was given to the question of how interests developed, or failed to, and why some were more readily mobilized than others.

David Truman, building on and sometimes departing from Bentley, did ask specifically how groups emerged. He specified an initial condition, the sharing of attitudes or interests among potential members, and suggested that these shared values would most often be brought into being by some exogenous disruption of a social equilibrium, such as war or technological change. Formal association for Truman, as for Bentley, was a matter of "mere technique," and not given special theoretical status, but as a practical matter the discussion of group emergence focused mainly on organized groups. Truman went on to suggest that group formation was often a "wave-like" process with organization begetting counter-organization until some new equilibrium was reached within the broad social sector in which the interests were located. In addition to this "homeostatic hypothesis," Truman presented a broader argument to the effect that groups proliferate as a consequence of the processes of social fission that result from the growing complexity of modern society (1951, p. 57).

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[2] I make no effort in this article to compare American patterns of interest representatives with those of other advanced industrial democracies. Membership associations with high rates of sectoral participation are considerably more common in European systems, and individual institutions may therefore have less room for assertive advocacy. Some aspects of "American exceptionalism" are treated in Salisbury (1979) and in Wilson (1981). See also Berger (1981).

Homeostatic and proliferation models both operate on a macrosocial level, and neither has anything directly to say about the specific formation of particular organizations. Truman assigned a functional meaning to group organizing—the stabilization of tangent relations—but he did not seem to regard the actual organizational process as problematic. He may well have shared the view that seems to have been the pervasive wisdom before 1965, namely, that like minded people join together to enhance their political power in order to achieve public policies that serve their common interests. It was this conventional view, of course, that Mancur Olson sought to redress.

Olson challenged the assumption that individuals would join an organization in order to press for public policies, the benefits of which they would enjoy whether they had joined or not. Philanthropy, coercion, or downright irrationality might lead some people to join some groups, but those considerations could not be expected to hold for the discernible profusion of extant organizations. It was Olson's central insight that interest groups, at least those concerned with economic interests, are not in the first instance organized for public policy-related reasons at all. Rather, they are constructed around the provision to members of selective incentives, material benefits unavailable outside the organization. Lobbying on public policy, if it occurs, is a by-product, an activity made possible by the internal exchange of selective benefits, but not necessarily given purposive direction by the values of the group's members. For example, a UAW member may or may not support the leadership's stand on a nuclear freeze or extension of the Voting Rights Act so long as his or her group membership is assured by a job and a union contract, and a doctor may read the *Journal of the American Medical Association* without sharing A.M.A. politics.

Walker (1983) has shown that in many cases some kind of subsidy, either from other groups or from the government, underwrites some part of the cost of creating and sustaining the organization. Nevertheless, one must still account for the ability of a group to attract members to that group and not some other, and for this problem Olson's argument still holds.

In subsequent revisions of Olson's original thesis, I (Salisbury, 1969, 1975) and others (Berry, 1977; Moe, 1980) contended that, although Olson excluded them, his argument applied fully as well to all the many groups that were organized around public policy goals. These, too, were vulnerable, highly vulnerable indeed, to the free rider problem. Why should any particular environmentalist join the Sierra Club or the Wilderness Society? Each organization must provide selective incentives, different from the competition and compelling enough to attract the potential free rider, or go out of business. These incentives might include a distinctive formulation of policy purposes—a better "line"—as well as some combination of material and solidary attractions. In more intimate settings interpersonal pressures may supplement the advantages of membership. Moreover, as Olson argued, relatively small groups may be created out of the self-interest of a few large powerful members willing to bear the costs of organization in order to gain the political advantages of group expression.

In all of these cases, however, the political strength of the organization is derived from the support of its members, whatever the means by which that support may have been secured; that is, we grant an organization legitimacy and pay attention to its policy requests because we assume that in some sense the spokesmen for the group represent the interests of the members. To be sure, this assumption is often challenged; we may charge the group with oligarchical tendencies, or in other ways suspect that the leadership is not representative of rank-and-file opinion. Many groups go to elaborate constitutional and procedural lengths to obviate such doubts and establish their *bona fides* as to the reflection of membership sentiment in the group's policy stands (see, for example, McFarland, 1976). The widespread use of mechanisms providing a "democratic mold" (Truman, 1951, pp. 129-139) clearly indicates that groups which are believed to represent organized membership constituencies have both a legitimacy in the policymaking arenas and potential clout, especially through votes and campaign funds, which "unrepresentative" organizations lack. Further, it is assumed that the policy interests expressed reflect either explicit or tacit concerns of the members, for otherwise they would leave the organization.

In fact, there are several modes of "interested activity" in which the assumption that group policy actions are driven by member preferences might be unwarranted. Under some circumstances, one example would be Olson's by-product situation. Conceivably the internal exchange of selective benefits might be so secure that no public policy inanity the leadership commits will alienate the faithful. Quite often group leaders espouse policy positions with only tenuous support from their members, although *severe* discrepancies between leader actions and member wishes are probably rare and generally short-lived. A second type of deviant case is the organization that is nothing more than a letterhead, lacking membership altogether. Interest groups to which no one belongs and which do not even provide for the possibility of membership are quite common among public interest groups, as Berry (1977) has shown. Similarly, Sorauf (1976, p. 19) has noted that in the field of church-state politics a good many "groups" are little more than institutionalized personalities. Leaders without organized followings may claim to have the sympathy of millions, of course, or at least to represent their "true interests," but such claims are likely to be received at a heavy discount.

At the other end of the spectrum there is the political movement, a congeries of organizations and individuals, participating in various ways in an effort to achieve a common set of policy goals (Asch, 1972; Gamson, 1975; Gusfield, 1963; McCarthy & Zald, 1973). It is characteristic of movements that many of the formal organizations within them have brief and highly volatile lives, and a large share of the sympathetically inclined individuals takes part only sporadically, if at all. Consequently, even though a movement may be very large in sympathy, it is typically uncertain in its mobilizable strength. Political movements are often characterized by great uncertainty also concerning who authentically speaks for those who identify themselves with the cause, with or without some formal membership, and typically there is considerable competition among several would-be spokespersons. Despite disputes over the correct line to take on a particular policy question, however, there is little doubt that the interests a movement represents are those of the "members," however that constituency is defined. Hence, all the norms and apparatus of representative democracy are applicable, and, in a society in which those norms are widely admired, that in turn places constraints on the tactical options available to movement leaders.

The several kinds of groups we have been discussing—political movements, voluntary organizations of members recruited through the use of selective incentives, and institutionalized personalities—all face the problem of establishing the legitimacy of their representational claims. The policy interests or values addressed are said to consist of the values of the group members, supporters, and identifiers. Mechanisms to allow consultation and display responsiveness are adopted. Moreover, since representational claims are so central to group legitimacy, the designation of group spokesman or representative is also a matter of critical importance. Many lobbying tasks can be performed by any competent hired hands, to be sure, but public articulation of group positions, if they are to be taken seriously, are inevitably caught up in the legitimation process and therefore cannot lightly be delegated or farmed out. The central point to be made is that in crucial respects the representation of membership-based interest groups differs significantly from the representation of institutional interests. Let us turn to the other side of this pairing to examine the difference further.

How Institutions Differ from Groups

In an earlier phase of the political science discipline a great deal of attention was paid to the theoretical meaning and position of institutions. Nearly all of this discussion, however, was focused on governmental institutions; the state and its parts and subdivisions. Very little mention was made of nongovernmental institutions except occasionally when it was noted that private groups also have governing structures and may exercise power over their members as well as influencing the larger society. Even this observation makes no distinction between voluntary associations of members and hierarchical structures which exercise authority over people within their jurisdiction. A corporation, a local government, most churches, and even universities are different, not totally but in crucial ways, from our conventional notion of interest groups, and the traditional literature on the nature of institutions does not tell us about the difference.

First of all there is the question of interests. We presume that people who join interest groups respond to selective incentives that appeal to their particular values and that whenever the incentives lose their appeal or their personal resources fail, they will drop out—voluntarily. Exit is obviously possible as well from any particular institutional setting (cf. Hirschman, 1970). One may quit one's job (or be laid off), move to another town, be graduated from school, or otherwise depart. Yet in most cases these acts send no message to the institution's leaders, nor are they intended to. To be sure, if half the population leaves, the city fathers may search for a new service formula; if a large number of students transfer, the college will be in trouble. Institutions of this kind must satisfy the needs and serve the interests of those who "belong." But when a corporation seeks to affect public policy—regarding pollution standards, for example—it does not justify its effort by alleging that it is reflecting the values of its employees. Nor does a university seek increased student loan funds on the grounds that its student body has expressed its desire for the money. It is not member interests as such that are crucial, but the judgments of organizational leaders about the needs of the institution as a continuing organization.

A central distinction between an institution and an interest group is that institutions have interests that are politically and analytically independent of the interests of particular institutional members. In part this derives from the continuing nature of a corporate institution. For example, it is presumed in both law and fact that a university has an existence transcending any agglomeration of individuals who happen presently to occupy its diverse roles. Inasmuch as students surely, and faculty often, are "mere birds of passage," trustees are admonished to be careful not to mortgage the future or rashly to spend the endowment. Similarly, the directors of a corporation can be held liable if they distribute to the stockholders everything not nailed down, with no thought of the long run. A voluntary association does not have the same expectation of eternal life. It must recruit members every year to stay alive.

Unless liquidation or merger destroys its identity, a corporate institution not only has a continuing existence, but it also possesses significant assets which belong to the corporate entity, not to the individual members thereof. It is from this foundation that institutional interests in public policy arise. Institutional leaders are charged with protecting, strengthening, and otherwise enhancing the assets of their institutions, both in the short run and to assure reasonable financial safety and stability for the institution in the future. Much of what they do has no direct political significance, of course. But in the latter twentieth century public policy is of such immense scope that issues continually arise which affect the assets of particular institutions, thus generating politically relevant interests (Salisbury, 1980). Environmental regulation, safety and health regulation, rules regarding employment practices, the host of grants and entitlement programs, tax policies—the list is long. Even an institution of the most modest size and aspiration will encounter both threats and opportunities in this policy array, and insofar as its resources permit may seek to increase its understanding of these possibilities and to influence them.

An institution can monitor public affairs and lobby to affect them only to the extent that it can afford to, of course. It must have resources available for such purposes. What we are calling institutions are generally complex organizations, highly differentiated and often with multiple functions. Such organizations typically command substantial and diverse resources and within limits a meaningful fraction may be allocated to policy-relevant tasks if and when these are perceived as useful to the maintenance and enhancement of the enterprise. Moreover, in most cases such allocations may be made without extensive consultations with members, employees, stockholders, or other constituent groups. They are, in effect, management decisions. And here lies a key distinction between institutions, as we use the term, and interest groups. Institutions are managed organizations. They are primarily hierarchical in their internal structures of authority, at least with regard to when and how far to become involved in the policymaking process. Membership groups must look far more carefully to the desires of their members, both to assure political legitimacy and to keep their supporters happy.

In distinguishing membership groups from institutions we do not mean to suggest that they are completely unlike. Most interest groups hope to survive into the indefinite future and thus take on institutional characteristics. Many possess substantial assets which they manage with a view to their long-term organizational health, and whenever public policy affects their institutional concerns, appropriate political action may be taken. Similarly, corporations and other hierarchies are not completely unconstrained by the views and needs of various groups both inside and outside the organizational boundaries (see, for example, Vogel, 1978). Demands for "socially responsible" behavior and for adoption of mechanisms to ensure democratic control are part of the political environment of all organizations in the United States, not just of membership groups. Nevertheless, there are important differences. Institutions have greater latitude—more discretionary resources and more autonomous leadership authority—to enter the political arena. Institutions have less need to justify their political efforts by reference to membership approval or demand. Institutions may also have a wider range of specific policy concerns. The instances in which policy impinges upon institutional interests will be numerous indeed for large complex organizations.

One result of these differences ought to be that institutions, not conventionally defined interest groups, will have come to dominate the roster of nongovernmental actors in Washington. This should follow inexorably upon the growth of government and of the scope and impact of policy. Before we turn to some examination of the empirical situation, however, let us consider briefly and in general terms the modes and motives of entering the political arena.

Entry into the Political Arena

Until the publication of Olson's *Logic,* the question of how and why groups undertake political activity was scarcely acknowledged to be problematic. Interest groups were essentially defined by their political presence, and although perceptive students clearly understood how much ebb and flow there was in the vigor and direction of political action, they gave it little theoretical attention. Olson showed that the mere existence of politically relevant shared values or interests could not account for the effective mobilization of those interests into politically relevant action; i.e., action aimed at effecting policy decisions that would supply collective policy benefits. Olson, as we have seen, did provide for the possibility that small homogeneous groups might so act, especially when one or a few members of the group had a particularly large stake in the collective good outcome. Otherwise, however, while persuading us that much lobbying had to be seen as a by-product of the internal organizational exchange of selective benefits, Olson did not explain why groups should bother to engage in political action. Why should the "by-product" of successful group formation be "spent" on political purposes rather than, say, on increased perquisites for the leadership?

One possibility is that securely entrenched group leaders act out of personal whim or conviction, unconstrained by member preferences, institutional needs or factional threats. Some of the late George Meany's positions on foreign policy come to mind in this connection (cf. Ràdosh, 1969), but it is probably very rare for leadership autonomy to be complete. More often, one suspects, group leaders lobby because they perceive potential effects of some government action on their members or members' values which the members might urge trying to influence if they were fully informed. A second and far more significant explanation is that group leaders are active on policy questions because that is why their members or followers support them in the first place. Here we speak of purposive or expressive groups. Despite Olson's reluctance to include such consumption values aong the incentives that organization offer their adherents, it is clear from subsequent empirical work, as well perhaps as being intuitively pleasing and logically persuasive, that members do often support organized efforts to secure collective public benefits, whether it is truly rational to do so or not. In any case, such purposive group activity presents no mystery regarding why political action is undertaken; it is of the very essence.

A third motive for action is institutional; that is, institutional leaders estimate that investment in political representation would be beneficial to the interests of the organization. One might treat this kind of decision as a rational calculus problem essentially like that faced by individuals in determining whether or not to participate politically, in groups or otherwise. Three considerations make it inadvisable to do so. One is that institutions are *far* more likely to be part of relatively small, similarly situated groups—Olson's privileged groups—and thus be able to organize more readily and to anticipate being more effective politically than most individual citizens can expect. Second, although a given individual may hold a large number of distinct values or interests and even embrace a considerable set of political causes, available resources for participation are quickly exhausted, and multiple modes of individual participation are comparatively uncommon. Institutions possess more resources which, combined with a greater sense of efficacy in political action, lead to a considerably increased probability of participation at any given level of intensity of interest or concern.

Finally, the very size and complexity of an institution renders it vulnerable to a much broader array of specific policy impacts, positive and negative, present and prospective. Indeed, insofar as individual citizens are themselves embedded for work and play in large institutional settings, as so many are, they experience many of the effects of policy only indirectly, through their respective institutions, rather than immediately and in person. This would largely be true for much of the "new regulation" of environment and job safety, for example, as well as for national programs affecting state and local government budgets and services. A given corporation is quite likely to find itself in several encounters at once, on different policy issues, being worked on in different institutional settings, and requiring different modes of political action. Perhaps individual level interests also possess such complexity in principle, but in actual practice they do not generate comparable diversity of interest representation.

The forms of political action undertaken by institutions are indeed diverse, although none is exclusively utilized by institutions rather than membership groups or even, in some cases, individuals. One common form is to join organizations of similarly situated institutions. Some of these are trade associations organized along specific industry lines. Others may be closer to "peak associations," attempting to encompass an entire sector of economic activity. A large research university might belong to several organizations of universities—AAU, NASULGC (if public) or NAICU (if private), AAMC (if they have a medical school) and other professional school groups, ACE (as the peak association for higher education), and perhaps some others (King, 1975). A manufacturing corporation may belong to several trade associations, depending on how complex its product line is, as well as the NAM, the U.S. Chamber of Commerce and the Business Roundtable. Part of these memberships may be expressive (the NAM would seem often to have been a forum for venting spleen regarding public policy rather than seeking realistically to affect it), and part may be more conventionally political, lobbying by small groups for beneficial collective policy goods.[3]

A mode of political involvement that has recently taken on Brobdingnagian proportions is the Political Action Committee (Conway, 1983; Malbin, 1980). Not all PACs have been created by institutions, but most of them have, and the prominence of PACs in campaign finance further illustrates how important institutional actors have become in our political life. Again, it must be stressed that although some PACs have raised significant sums from individual enthusiasts of liberal and, especially, conservative causes, business corporations, trade associations and, to a much lesser extent, labor unions dominate this form of activity, constituting nearly 78% of 3,371 members active at the beginning of 1983 (*National Journal,* 1983).

Finally, institutions enter the political arena directly. Several thousand corporations maintain permanent Washington offices to monitor the political scene and to help represent organizational interests.[4] Corporate divisions of public or government affairs are standard organizational components, not only of business enterprises but on a smaller scale of universities and many other institutions of contemporary American society. Further, many of these institutions retain outside firms, primarily lawyers and public relations counsel, to represent them in Washington in particular aspects of their wide-ranging institutional encounters with the federal government. The key word here is "represent," for it carries some current meanings that have not adequately been incorporated into the formulations of political science discourse in representation. Let us turn next to that issue.

Washington Representatives and the Nature of Representation

It is rather startling to pick up a large volume entitled *Washington Representatives, 1982* (Close, 1982) and find that among the thousands of individuals listed, not one is a member of Congress. Yet, although these representatives are not accorded that status by customary formulations of social scientists and philosophers (Eulau, 1952, 1967; Pitkin, 1967; Wahlke et al., 1962), they cleary are so designated by the world. There are some differences in functions and activities, of course, between nongovernmental representatives and those who hold authoritative office. Let us consider of what these differences consist.

First, an official representative is said to have a constituency to which he is (or is not) responsive. An elected representative has a legally defined constituency, but he also has an effective one, those who supported him and those whose preferences he actually seeks to advance in policy-related actions. In this second sense, every official may be said to represent a constituency, i.e., to reflect some values rather than others in decisions. A "Washington Rep," on the other hand, typically represents a client, an organization, group, or individual (and occasionally only himself) who retains the representative and defines the scope of interests to be represented. The concept of client ordinarily implies a narrower range of policy concerns and a more limited set of representational activities than are contained in the idea of constituency representation. A corporation may retain a lawyer to represent it before the Federal Communications Commission on a specific licensing case, or alternatively to monitor the Federal Energy Regulatory Commission decisions on a continuing basis. Client representation may take many forms, but generally it is more specialized in subject matter, more limited in both scope and time, than constituency representation. At the margin the differences grow indistinct. The AFL-CIO is represented in Washington by a substantial crew of people and is actively interested in a range of questions that very nearly matches the full congressional agenda. Nevertheless, labor lobbyists have broad discretion to choose whether or not to get actively involved, whereas congressmen can hardly pick and choose so as to avoid all the issues about which their constituents disagree or are indifferent.

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[3] Trade associations present some special complexities for our argument. They themselves are membership organizations, but their members are corporate institutions. The members must be kept satisfied with association policy, selective benefits of other kinds, or both, but being institutions, their calculus regarding membership benefits may contain considerable slack. Moreover, corporate institutions may seldom confine their policy-related activity to association membership. That trade associations do operate under member-induced constraints has recently been illustrated by the cutbacks in association staff and programs brought about as a consequence of the economic recession (Teeley, 1983). Trade associations, like other groups, are often fragile organizations financially, begun with very shadowy support out of an enterprising lawyer's office, or on some otherwise flimsy basis. One result of this is the appearance of association management companies, interest group wholesalers, which manage the affairs of several small organizations. For example, Smith and Bucklin, a Chicago firm, operates more than 70 trade associations, and the National Center for Municipal Development assists dozens of local governments in their quest for federal support.

[4] Governmental affairs offices have existed for a considerable time, of course. The Public Affairs Council, an organization composed mainly of corporate government affairs officers, was formed in 1954, and the Brookings Institution sponsored a roundtable discussion among 19 of them in 1958 (Cherington & Gillen, 1962). Little notice has been taken by political scientists, however; the Cherington and Gillen volume was never reviewed in the *American Political Science Review.*

Dexter (1969) discusses Washington representatives in some detail, but unlike his earlier work (Bauer, Pool, & Dexter, 1962), this book does not seek to tie his discussion into more general theories of interest group performances.

Client representation is relatively specialized and often quite discontinuous. Any specific representative may work on behalf of a particular client for a purpose that has a short life of relevance. Often, moreover, the representation is directed toward a specific agency, committee, or other unit of government, and once the policy issue moves away from that institutional setting to another, a different representative more familiar with the new arena may be brought in. Thus, specialized client representation often results in a given client retaining several representatives to perform highly focused representative services. The corollary of this is that a given representative often specializes in working with a particular piece of the governmental policy machinery and may represent numerous clients with problems involving that agency or committee.

Implicit in the foregoing discussion is the notion that representation involves three essential components rather than two, as conventional discussions of the topic assert. In addition to the representative and the represented, there is also the agency of government to which the representation is directed. Actually there are several prepositions employed to describe this side of the relationship. A lawyer may speak for a client *to* a congressional staffer; he may testify *in* court, *before* a judge; he may negotiate *with* an agency over a compliance schedule; indeed, he may only monitor what government does and counsel the client, in which case no prepositional connection with government is made. Even this last is seen as a form of representation, however, and requires the third party's presence to be meaningful.

It is not clear whether a third party is necessarily implied in traditional conceptions of representation. It is rarely if ever mentioned explicitly, but apart from descriptive representation, which can occur simply by possessing the appropriate personal characteristics, representation of constituency interests cannot logically occur unless there is some sort of "other." A legislator does not achieve his or her constituents' interests or even express them meaningfully without official status in an institutional arena where negotiations may be undertaken to accomplish his or her representational purposes. Nevertheless, ordinary speech vocabulary does not force us to recognize the institutional arena as a separate element in constituency representation as it does in considering client representation.

A full analysis of Washington representation requires us then to identify individual clients as well as broader constituencies, to examine who the representatives are and the diverse terms and conditions governing their service, and to understand the specific governmental institutions that serve as the focus of any particular representation activity. As we have said, much of that activity, although by no means all, is highly specific as to client, representative, and agency. It is often short-lived in duration and limited, even imperceptible, in its impact on any part of public policy or the world. Representational activity is very often ineffectual, redundant, or otherwise useless. Moreover, like the dog that Sherlock Holmes realized had not barked in the night, effective representation may entail doing nothing. In any case, however, this conception of representation is broader and more highly differentiated than either of two traditions in mainstream political science to which it is related, legislative representation and interest group lobbying.

Nevertheless, there remains a profound difference between the governmental official who, in either a Bentleyan or a Burkean sense, represents some interest in a specific authoritative choice among policy alternatives and the nongovernmental representative who can never do more than advocate that interest. Each may represent a client or constituency, but there remains a significant gulf between their respective capacities to act. Thus, even though only one key word is used, there are two quite different component processes involving two quite distinct sets of people, a nongovernmental set engaged in policy advocacy and a governmental set engaged in policy determination. If ordinary parlance calls both groups representatives, can we mitigate the confusion somewhat by using sharply distinct terms for the two processes? The answer is probably yes, but not if the advocacy process is called lobbying. That much-abused word is so fraught with ordinary language meaning, most of it unsavory, as to defy rehabilitation anyway, but it is also true that none of its historic uses comfortably fits what many Washington representatives do. The word lobbying does not well convey the meaning of a presentation by a drug company representative seeking approval of a new drug from the Food and Drug Administration. Nor does it capture the character of discussions between a committee on technical standards of the Aerospace Industries Association and Pentagon procurement officials. *Amicus curiae* briefs presented to the Supreme Court have sometimes been called "lobbying" instruments (Barker, 1967; Krislov, 1963), but they are different in much more than tactical form from the efforts of the National Rifle Association to prevent gun control legislation or of Chrysler to secure financial help. The latter are surely examples of lobbying; amicus briefs and FDA appearances involve a much more restricted kind of policy advocacy, by representatives acting in behalf of clients.

Interest representation is an encompassing term that embraces lobbying in its ordinary use as well as narrower, more rule-bound forms of policy advocacy. Interest representation incorporates the client-representative relationships of a corporation and its lawyer, the membership group and its president, and the congressman and his constituents. Within that conceptual frame we distinguish governmental officials from those outside, policy formulation and enactment from advocacy. Within both those distinctions we may wish also to distinguish among specific institutional settings in which representational activity occurs. The forms of policy advocacy in courts are different from those in legislatures and so are the forms and norms of policy making. One reason that the term lobbying is so awkward is that it was developed initially to characterize policy advocacy in and around legislative settings where its connotations made sense and only later applied to other contexts. In any case, we should try to establish a vocabulary that is compatible with observable political life and is as consistent and straightforward as possible.

If we turn from a focus on process to a focus on role, we find, to begin with, the same confusion between lobbyist and representative as between the equivalent verb forms. The two words have vastly different connotations in the American lexicon but frequently refer to the same phenomena. Again, lobbyist more often is used with reference to policy advocacy in and around legislative bodies (Milbrath, 1963; Ziegler & Baer, 1969). A less clearcut but not uncommon distinction uses *lobbyist* to refer to an organizational employee, subordinate to those who make policy for the organization, whereas *representative* connotes a free-standing agent, retained on a fee-for-service basis and often on the assumption that the agent possesses particular skills or credentials of relevance to the advocacy role that are not readily available within the interested organization, whether institution or membership group. Even when we do not distinguish between the two *words* in that manner, however, the two *roles* are distinguishable and of both theoretical and descriptive interest.

As a first approximation of our descriptive needs it will be helpful to know what proportion of the very large community of Washington representatives are employees of the organizations whose interests they represent. Milbrath (1963, p. 150) found that as of 1956, 74.6% of the 114 registered lobbyists he interviewed were employed by the organizations represented. Of the remainder, 21% were outside lawyers, hired on a fee basis, and 4.4% were non-lawyer consultants. Table 1 reports comparable data compiled from the listings in *Washington Representatives,* 1981 in four broad policy areas, selected to reflect reasonable diversity in subject matter and styles of representation. Not only are the 1981 proportions remarkably similar to those Milbrath found 25 years before, they are essentially constant across the four policy domains.

This consistency is all the more impressive when we remember that it has survived an explosive growth in the numbers of groups and institutions seeking Washington representation as well as in the numbers of individuals providing it. Estimates of raw numbers for earlier years are neither plentiful nor, probably, very reliable. Schlesinger (1949, p. 46) claimed that in 1942 some 628 organizations maintained offices in Washington, whereas Albig (cited in Blaisdell, 1957, p. 59) counted 1,180 organizations located there in 1947-1948. By 1981 there were approximately 1,600 trade and professional associations, about 100 labor unions, over 200 individual states, cities, counties, and other units of government, numerous foreign governments, more than 4,000 corporations, and membership interest groups numbering well over 1000, all located in the Washington area. Temporary and ad hoc issue coalitions and other short-lived organizational efforts make it difficult to establish exact magnitudes, but it seems reasonable to suppose that the contemporary Washington community of interest

Table 1. Organizational Forms of Washington Representation

	Agriculture	Labor	Health	Energy
Law firms	17.9%	18.1%	16.8%	15.9%
Consulting and public relations firms	6.6	4.9	6.4	5.7
Corporations, trade associations and other institutions	75.5	77.0	76.8	78.4
N	832	304	722	1574

Source: *Washington Representatives, 1981.*

Table 2. Lawyers and Non-Lawyers, In-House and External Representatives

	Lawyers		Non-Lawyers	
	In-house	External	In-house	External
	%	%	%	%
Agriculture	12.4	79.4	87.6	20.6
Labor	8.3	71.4	91.7	28.6
Health	15.2	84.1	84.6	15.9
Energy	18.9	73.7	81.1	26.3

Sources: *Washington Representatives, 1981;* Martindale-Hubbell.

representation includes more than 7,000 organizations in more or less permanent residence.

When we turn to individual representatives, the numbers are even more startling. In 1981 the total capital area employment of trade and professional associations headquartered there was estimated to be more than 40,000 (Helyar, 1981).[5] In addition, as is well known, the number of lawyers in Washington has grown enormously. Membership in the District of Columbia Bar Association became mandatory in 1973, and between then and September, 1981, it went from 10,925 to 34,087. This number is somewhat misleading. Significant numbers of lawyers belong to the D.C. Bar who live elsewhere in the country. At the same time, however, a considerable number of people with law degrees who work in Washington do not actually practice law; many work for the federal government. Hence, the total number of lawyers residing in the capital may well exceed 40,000.

We may easily be overwhelmed by these numbers, and they are truly impressive, but we should remember that by no means all trade association employees or all lawyers in Washington are really representing politically meaningful interests. A sizable percentage of the lawyers are engaged in the private practice of private law. The so-called "Fifth Street Bar," for instance, centers around the local District of Columbia courts, and its members never have occasion to lobby on Capitol Hill. Similarly, many association employees are engaged in internal administration and support and are not really relevant to our concerns here. On the other hand, an unknown but substantial number of representatives come to Washington from elsewhere, some often and some only occasionally, advocating policy positions but never becoming part of any register or roster of representatives. If we look only at the Washington-based individuals actually performing policy representation roles, again drawing on our examination of the listing in *Washington Representatives,* we can observe that in our four broad policy areas, 4,227 individuals were listed, of whom 35.6% were lawyers. This proportion is remarkably close to that reported by Milbrath and also to the proportion of lawyers among public interest group lobbyists of the early 1970s examined by Berry (1977, p. 88). In the 1981 listings the labor area had a significantly smaller percentage of lawyers (17.1%), but in the other three policy domains the lawyer presence ranged from 31% to 39%.

We pay particular attention to the prominence of lawyers, in part because of the considerable publicity that has been given to them in recent years, but in greater part because the concept of the lawyer as representative carries some interesting connotations. Lawyers traditionally and predominantly operate as free-standing professionals retained by clients for relatively specific defined services and compensated by fee. Although many lawyers actually work in large organizational settings on a salaried basis, we would still expect that lawyers will, more often than non-lawyers, be found outside the organizations retaining them. Table 2 indicates that this is indeed true and that the proportions of lawyers who are in-house employees remain quite similar across the policy domains.

One consequence of the large number of lawyers around Washington may well be increased specialization in the representation process, possibly because lawyers are typically hired as outside counsel. Their services are sought when, and only when, the client firm requires those services, and their billing is ordinarily based on hourly charges. In-house counsel might work on anything the organization requires, but external representatives typically have a more limited assignment in both the time to be given and the official governmental segments to which the representation is made. A law firm might attempt to provide full representational services to a given client, advocating policy positions before all the

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[5] Twenty-nine percent of all such organizations are located in the Washington area, up from 19% in 1971 (Close, 1981).

Table 3. Agricultural Interest Representatives in Washington

	Washington Representatives, 1981	Congressional Committee Testimony, 1977, 1979, 1981	*New York Times* References, 1977-1982
Business corporations	33.5%	10.3%	16.8%
Rural cooperatives	4.3	10.7	6.3
Governmental institutions	1.5	12.8	5.2
Non-profit private institutions	.4	6.4	4.7
Foreign interests	12.2	.6	1.6
Business trade associations	29.8	16.0	14.1
Farm organizations	9.9	30.7	22.5
Citizens groups, including labor	6.3	9.5	27.8
Not classified	2.1	3.1	1.0
Total	100.0%	100.1%	100.0%
N	466	2579	191

Sources: *Washington Representatives* (Close, 1981), Congressional Index Service, *New York Times* Index Service.[6]

legislative, executive, and judicial agencies that affect the client's interests, but no individual representative can hope to do so. The ever-increasing scope and variety of government and policy impact make this strategy a difficult one for even a large firm to employ successfully, however, and the more common response is the use of multiple representation by a given client. Ford, General Electric, IBM, GM, Exxon, and other giants may hire eight or ten separate outside representatives in addition to working through trade associations and their own substantial governmental relations staffs. The government of Japan in 1982 retained nine different representatives for its embassy and four for other parts of its government, while several dozen others work for specific Japanese firms and associations (Madison, 1982).

The heavy reliance on representatives, typically lawyers, chosen to speak to government officials on behalf of a limited set of client interests is more characteristic of institutional representation than of membership groups, especially when the membership is composed of individuals. Such groups seldom can afford the investment in hourly legal fees that such specialized representation calls for, and they are much more likely to adopt a jack-of-all-trades style. The sheer financial scale of institutional representation vastly exceeds the magnitude of purposive group activity, and the respective personnel pools reflect this differential investment level. Only about 1,200 of the nearly 8,000 individuals listed in the 1982 *Directory* worked for purposive groups, and as numerous case studies attest, the salary and support levels enjoyed by this disparate group of advocates are hardly commensurate with the resources available to most institutional representatives.

We can round out this part of our discussion by reporting one other set of data demonstrating the importance of institutional representatives, especially of business corporations. For a single policy area, agriculture, Table 3 compares, first, the representational presence in Washington of different kinds of organizations declaring significant concerns with agricultural policy; private business corporations, rural cooperatives, governmental institutions, nonprofit private institutions, foreign interests, agri-business trade associations, general farm organizations and commodity groups, and other groups with purposive interests in agricultural policy. Only the last three categories are in any sense membership groups, and only in the last two sets are individuals enrolled. Thus, individual membership groups constitute only one-sixth of the community of agriculture interests more or less continuously represented in Washington. It should not be assumed that all individuals listed are equal in political importance or policy input. On the other hand, the representational presence reflected in column 1 permits a wide array of interest-serving activities to be undertaken. Informal lobbying, monitoring and grass-roots mobilization, and representation before administrative agencies are all greatly enhanced by an organizational presence on the scene. Rather than organizations of farmers, it is institutions, especially those of business, which clearly dominate that presence.

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[6] References in congressional committee testimony and *New York Times* references were calculated by reference to keywords "agriculture" and "farm," combined with "policy" and "legislation." *New York Times* references covered the period from January 1, 1977 to June 30, 1982.

In more public and visible arenas, however, the picture changes. In testimony before congressional committees on matters of farm policy (column 2), farm organizations play the most prominent role. Newspaper mentions of organizations in connection with farm issues (column 3) are even more fully dominated by membership groups. Business corporations and trade associations recede. The lesson to be drawn is partly one concerning legitimation. The public process of policymaking gains more legitimacy through hearing and appearing responsive to self-interested individuals and groups than by deferring to organized institutions and institutional interests. In addition, however, it is probably the case that much of the substantive policy concerns motivating institutional advocacy consists of quite small items of no interest to most groups of farmers or the general public. This is especially likely when, because of the multiple representation phenomenon we have described, a corporation or trade association retains a representative for a single, highly specialized task. This kind of thing counts in column 1 but is not otherwise noticed. When all the small specific policy items are added together the list can be formidable indeed, of course, but few of them may have attracted the attention of the *New York Times* or congressional committee hearings. Nevertheless, we can surely say that for much of what happens in Washington to influence the authoritative allocation of scarce resources in agricultural policy, the general farm organizations, such as AFBF, NFU, Grange, and NFO, once thought to be so powerful, are less prominent than conventional textbook treatments suggest.

The importance of institutional representation has some significant implications for the likely course of public policy as well as for our theories of how and why things work. The use of multiple representatives, each concentrating on a specialized set of policy concerns, surely must intensify the fragmenting, disaggregating tendencies in public policy so often alluded to in recent literature. Secondly, institutional representation may be expected to be far more durable and persistent in policymaking circles than most purposive groups or even membership groups based on material incentives. Institutional resources are not infinite, of course, but neither are they as often subject to membership demands for review or as vulnerable to shifts in political tides and entrepreneurial fortunes. Institutional representation may be more durable, but it is also typically more prosaic, more pragmatic, than purposive group advocacy. Institutional interests more often concern export licenses than anti-Communism, city budgetary assistance more than a war on poverty. (Corporate PACs continue to give significant financial support to their supposed ideological enemies, liberal Democrats. Surely this is a prime example of the pragmatism of institutions (Handler & Mulkern, 1982, esp. pp. 20-27).) In a very important sense, the conservative bias that Schattschneider (1960), McConnell (1966), and other critics of pluralist America have long attributed to interest groups is rooted far more in the power of institutional representation than in conventional membership groups. Indeed, it is the comparative weakness and fragility of membership interest groups of every point of view and persuasion, not their strength, that may be argued to be responsible for whatever malaise the American polity has lately suffered from interest-based politics.

This effort to clarify the conceptual map of American policymaking processes has yielded three main conclusions. First, I distinguish between institutions, institutional interests, and their representation, on the one hand, and what we have rather awkwardly called membership interest groups on the other, and I argue that important theoretical differences exist between the two. Second, I note that both these organizational forms of nongovernmental interests are represented in Washington by large numbers of people whose roles have many conceptual similarities to those in formal policymaking positions and who, like their official counterparts, are called representatives in everyday usage. Nevertheless, there remains a critical difference in status and function between policy advocates and policymakers which the fuzziness of language should not be allowed to obscure. Washington terminology is not more precise than that used by political scientists to describe and analyze what happens there, but it is somewhat different and in recent years has grown more so with regard to interest representation and policy advocacy. Finally, we urge that the power and scope of institutional representation is such that our teaching, our research and our normative evaluations will all go awry unless we make the appropriate corrections in analytic focus.

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Nationalization of the American Electorate

WILLIAM CLAGGETT
The University of Mississippi

WILLIAM FLANIGAN
University of Minnesota

NANCY ZINGALE
College of St. Thomas

The conceptual meaning of and the empirical evidence for the nationalization of the American electorate is explored. Two conceptually distinct dimensions of nationalization are identified: nationalization in terms of a convergence in the level of partisan support across geographical subunits of the electorate, and nationalization in terms of a uniformity of response by geographical units to political forces. Empirical estimates for both types of nationalization are derived for the American electorate for the period from 1842 to 1970. Unlike previous scholars, we find no trend toward increasing nationalization of the vote. Possible reasons for this lack of nationalization of the vote are suggested.

A widely accepted observation among political scientists holds that the American electorate has become increasingly nationalized over the past one hundred years or more. In other words, the current electorate is seen as more attuned to national level events, personalities, and issues, and hence is more homogeneous in its behavior than the electorate of the nineteenth century or the first half of the twentieth century. The argument has often been cast in terms of presidential elections, citing, among other things, the growth of the national media, which present a similar image of the candidates in many different localities, but it has been applied to congressional elections as well. Presidential coattails benefitting congressional candidates everywhere, midterm congressional elections as a referendum on the performance of the incumbent president, and the emergence of national party images in such areas as civil rights, limiting the ability of congressional candidates to dissociate themselves from national party fortunes, have all been suggested as manifestations of the decline of localism and rise of nationalization of American politics.

Perhaps the most influential work in advancing this proposition was done by Stokes (1965, 1967), who sought to measure by analysis of variance techniques the relative magnitudes of national, state, and constituency effects on turnout and vote choice in congressional elections. In the first of these articles, Stokes found that national level forces contributed far more to the variance in turnout during the 1950s than either state or congressional district forces. In contrast, district forces accounted for the largest proportion of the variance in the partisan division of the congressional vote, with the national effect a close second (Stokes, 1965, pp. 75-78). Later, in a longitudinal and comparative study, Stokes concluded that the American system had become increasingly nationalized since the Civil War. His major evidence was a steady century-long decline in the impact of constituency forces on congressional turnout and a somewhat smaller and shorter decline in the impact of constituency forces on the partisan vote (Stokes, 1967, pp. 192-198).

Using less sophisticated methodology, other researchers have echoed or, in one case, foreshadowed Stokes's findings. In *The Semisovereign People,* Schattschneider argued that the significance of 1932 lay in the replacement of a sectional with a national political alignment and the consequent nationalization of politics (Schattschneider, 1960, pp. 78-96). Similarly, Sundquist saw nationalization in the continuing convergence of party strength across the country since the 1930s (Sundquist, 1973, pp. 332-340). Sorauf's analysis of presidential elections led him to a similar conclusion although he dated the trend as beginning in 1896 (Sorauf, 1980, p. 51).

Perhaps because they seem so consistent with the decline of parochialism and localism under the onrush of modernization, these findings have become part of the core of accepted knowledge in political science and utilized as such by other scholars (see, for example, Converse, 1972, p.

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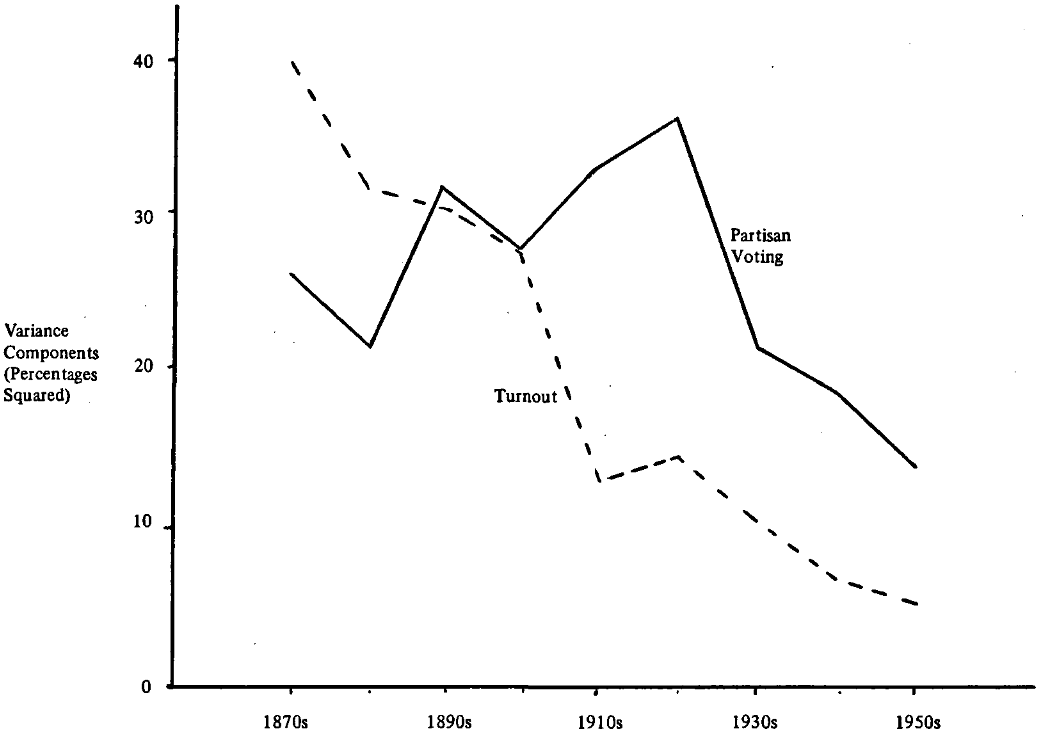
Figure 1. Constituency Influence on Turnout and Partisan Voting, 1870s to 1950s, as Presented by Stokes



Source: Donald E. Stokes, "Parties and the Nationalization of Electoral Forces," in William N. Chambers and W. Dean Burnham (eds.). *The American Party Systems* (New York: Oxford University Press, 1967), Figure 4, p. 195.

268; Niemi & Weisberg, 1976, pp. 447-448; Polsby, 1981, p. 29).

Other research, not directly concerned with nationalization, suggests that these conclusions may require another look. The recent increase in the incumbency effect and the decline in presidential coattails in congressional elections suggest that the current electorate is still capable of idiosyncratic and localized behavior. For example, in his analysis of congressional voting, Mann concluded, "We are witnessing an increasing localization of political forces in congressional elections" (Mann, 1978, p. 102). Furthermore, Pomper's (1980, pp. 87-91) examination of the homogeneity of shifts away from normal levels of support by states in presidential elections also found little in the way of a trend toward a more nationalized electorate over the last century.

If we look again at the data that Stokes presented, further questions arise, particularly if we consider separately the evidence on turnout and on partisan voting. Stokes's data on constituency effects for turnout and partisan voting in congressional elections from the 1870s to the 1950s are shown in Figure 1. The data on turnout are thoroughly convincing, showing a dramatic decline in constituency effects from 39.8 in the 1870s to a score of 5.2 in the 1950s. There is much less of a trend for partisan voting; in fact, there may be two weak trends in the partisan vote—a long upward trend and a shorter, downward trend.

The more appropriate evidence on increasing nationalization, however, would be the trend in the national effect, since a decline in the constituency effect does not necessarily imply an increase in the national effect.[1] Stokes did present data on the national effect for turnout and, although not as smooth as the constituency effect, overall an increasing trend can be seen (Stokes, 1967, p. 194, Figure 3). However, no corresponding data were presented for the national effect for partisan voting, although a footnote promised such data in a future article (Stokes, 1967, p. 196).

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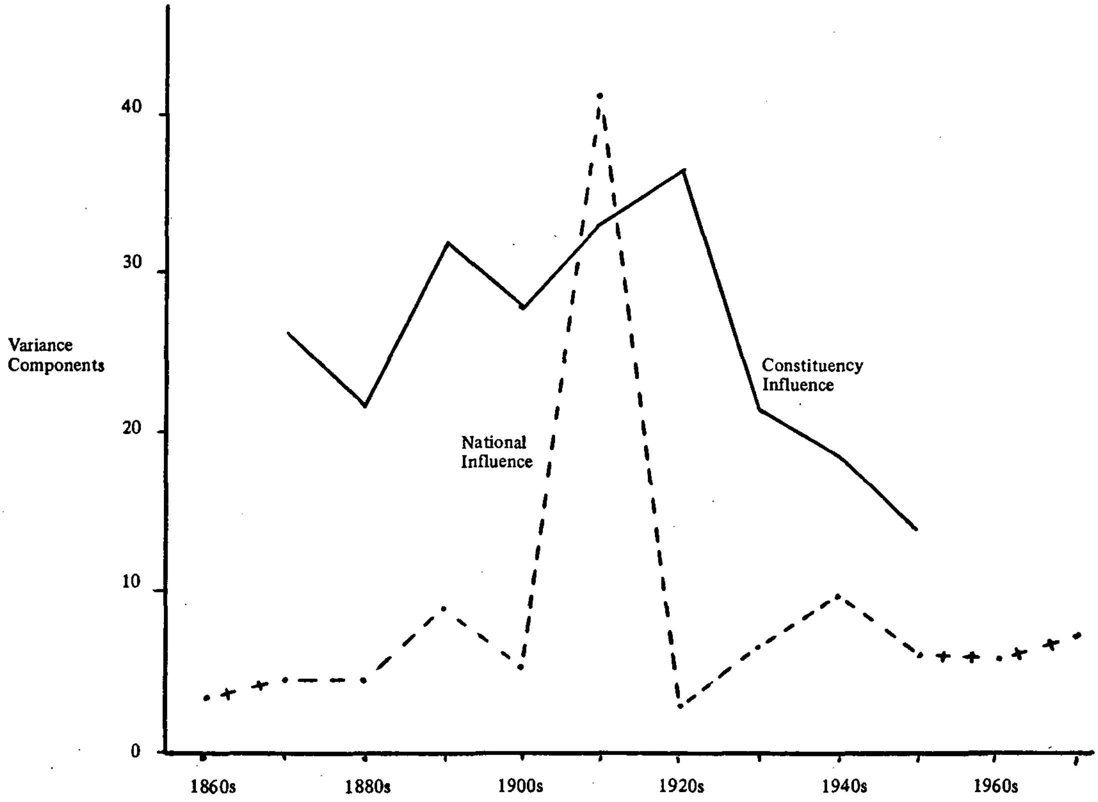
[1] The magnitude of the national, state, and constituency effects are all independent of each other. (This, of course, would not be true if the data were normed to

This national effect for partisan voting in congressional elections can, however, be calculated very simply with the use of Stokes's method, and we have added those data to Figure 2 along with the constituency component for partisan voting as originally calculated by Stokes.[2] It is clear from Figure 2 that there is no substantial increase in the national component over the years from 1872 to

Figure 2. Constituency Influence on Partisan Voting as Presented by Stokes, 1870s to 1950s, and the National Influence on Partisan Voting, 1860s to 1970s



Source: Stokes's data same as Figure 1.

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take account of fluctuations in total variation as is done in Figure 5 below.)

[2] The national component was calculated simply by using the national Democratic percentage of the two-party vote in each election as the basis for estimating the variance around the mean of the entire decade. This calculation of the national effect is simple enough, but the national vote totals for each party are not precisely the data Stokes used. He would have eliminated all constituencies in each decade that did not have a Democratic-Republican congressional race in every election. The national totals we used include those data.

We made a second set of estimates which excluded uncontested races but used percentages of total vote, and this showed an overall *decline* in the national effect from 1872 to 1970. Our estimate of the national effect was 9.35 in 1952-1960 and Stokes's was 9.32, which may mean we eliminated data in the same way. We decided it was more cautious to present the data in Figure 2 which show no trend rather than a decline in national effect.

Also, by dividing the data at 1932 a large national effect is ignored between the late 1920s and early 1930s. Using estimates for every set of five elections would not, in any case, lead to the conclusion that the national effect is increasing during the twentieth century.

When using percentages based on the two-party vote only, as Stokes did and as we present in Figure 2, it does not matter which party's percentages are used for this analysis. The results are the same for either party.

1960. The hashmarked lines before and after these years extend the estimates of the national effect in both directions beyond the years analyzed by Stokes, but these additional data do not alter the conclusion that nationalization in this form has failed to increase. With the exception of the turbulent period from 1912 to 1920, the national component is best described as erratic and low.[3]

At this point one must conclude that there is no nationalization of partisan voting at work in congressional elections. Swings in congressional voting continue to be more influenced by factors within the constituencies, and the impact of national forces on congressional vote choices has not increased over the past century or so.

Note, however, that Stokes's original conclusion (1965) was very close to this—that turnout was much more nationalized than the party vote. In his 1965 article Stokes wrote:

> The magnitude of the national and state turnout components make clear that vast numbers of people are drawn to the polls (or kept away) by statewide or presidential races; *turnout* coattails are cut from tough material. And yet as he votes for Congress, the elector responds much less strongly to partisan forces originating in these other contests. . . . *Partisan* coattails, apparently, are cut from lighter cloth. (Stokes, 1965, p. 78, emphasis in original)

Only later did he argue that the party vote was also subject to increased nationalization over time. In his comparative study of Britain and the United States, he wrote:

> The primary conclusion to be drawn from the turnout and partisan components in the United States over most of a century is the substantial decline of the constituency as a distinct arena of conflict in congressional elections. (Stokes, 1967, p. 196)

However, it is this latter conclusion, which we think is based on incomplete data, that has worked its way most thoroughly into the literature.

Still, the notion of increasing nationalization of American politics remains theoretically attractive. Surely, improved communications, a national economy, national media, and nationwide campaigning by presidents for congressional candidates of their party *ought* to have produced a more nationalized response by voters. The idea makes too much intuitive sense to abandon cavalierly. With this in mind, we intend in this article to take another careful look at the issue of nationalization: its conceptualization, its measurement, and its empirical contours.

Conceptualizing Nationalization

As we look at the way in which the concept of nationalization has been used by scholars, the term seems basically to mean a *uniformity* or universality of political behavior across all areas of the nation. (One exception to this will be noted below.) Within this common emphasis on uniformity, however, two quite distinct aspects, possibly dimensions, of the concept can be discerned. On one hand, nationalization has been seen as *convergence in the levels of partisan support* across the nation, thereby producing a more uniform electorate geographically. On the other hand, nationalization also has been viewed as uniformity in the *response* of geographical units to the political forces in an election.

Nationalization as Convergence in the Levels of Partisan Support

Levels of partisan support converge when the parties draw an increasingly uniform level of support across all geographic subunits of the electorate; that is, nationalization involves the erasing of regional or state differences. Although it may not mean the homogenization of the electorate, it suggests that distinctive regional political cultures and traditions are being replaced by a more similar mixture of political sentiments across the nation.

This interpretation of nationalization is explicit in E. E. Schattschneider's assertion that 1932 was a watershed in the nationalizing of American politics. He wrote:

> Elections since 1932 have *substituted a national political alignment for an extreme sectional alignment everywhere in the country except the South.* Graphically, the nationalization of American politics can be seen in *the flattening of the curve showing the percentage distribution of the major party vote outside the south.* (Schattschneider, 1960, p. 89, emphasis in original)

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[3] Although it was a simple matter to add the national component, it would be more difficult to reconstruct the state effect, and we have not attempted to do so. Regardless of the pattern demonstrated by the state component over the years, it could not alter our estimation or interpretation of the national effect.

The varying size of a state component could alter our comparative evaluation of national and district effects, but it is reasonable to suppose that the state component has been relatively small and not particularly variable. In the 1952-1960 decade for which Stokes reports a variance component for the state level the value was 5.32 in contrast to 9.32 for the national level and 13.98 for the district.

Nationalization in this sense also underlies Sundquist's (1973) discussion of the convergence of partisan strength since the 1930s and Sorauf's work (1980), which shows convergence since 1896. For example, Sundquist stated that, "The distribution of the population of basic attitudes toward the aligning principle of the New Deal party system . . . presumably does not differ greatly from region to region and from state to state" (Sundquist, 1973, pp. 332-333). And later,

> At that point [after convergence], the whole nation will be in a system of competitive politics in a roughly uniform pattern with an approximately equal balance between the parties. The regional distortions embedded in the party system in the 1850s, solidified by the Civil War and exaggerated by the realignment of the 1890s, will have finally been eliminated. The party system will have been nationalized and aligned on the basis of issues of the twentieth rather than the nineteenth century. (Sundquist, 1973, p. 340)

Three points should be noted about this conceptualization. First, there is implicitly a dynamic element in the idea of convergence. Since we would not require exact similarity among geographic units to satisfy the criterion for nationalized politics, we think instead of an increasing similarity among units over time. Thus the measurement of convergence requires a lengthy time series to demonstrate the progressive nationalization of American politics. Schattschneider and Sundquist essentially compare the similarity in election outcomes among states in the 1920s with the similarity in the post-New Deal period. Although more extended, Sorauf's analysis is also confined to the twentieth century. If the level of similarity or dissimilarity in the fourth party system (1896-1928) is somewhat deviant, it is possible that the trend observed since that time may not characterize the entire sweep of American electoral history.

Second, and more important, nationalization as the convergence of levels of partisan support is inherently an aggregate concept. It represents the increasing similarity of geographic units and hence the lessening of sectional cleavages, not the increasing of individual similarity nor the erasing of individual differences in the electorate. This raises questions, however, about the appropriate aggregate unit of analysis since there are several levels of aggregation that might be employed. Schattschneider, Sundquist, and Sorauf all used states as their units of analysis, but one might well argue for regions if the deterioration of a truly sectional alignment is being considered, or, on the other hand, for a smaller unit such as counties, since a considerable amount of geographic heterogeneity could be masked in statewide totals.

Finally, in both Schattschneider and Sundquist's work there appears to be some confusion between increasing nationalization in this sense of convergence in the levels of support and increasing competitiveness of the party system. These phenomena are not necessarily connected. All geographic units in a political system might be divided 80 to 20% favoring one party—highly uniform levels of support, but in no way competitive. Schattschneider in particular used measures that tap competitiveness more directly than uniformity in levels of party support and comes close to using the terms nationalization and competitiveness interchangeably (Schattschneider, 1960, pp. 89-92). In neither Schattschneider's nor Sundquist's writing is it entirely clear whether the discussion of competitiveness is a commentary on the conceptual properties of nationalization or the empirical observation that competitiveness has been a correlate of the current trend toward nationalization in American politics. Our own preference is to maintain a conceptual separation between nationalization and competitiveness since we can easily imagine national alignments and national cleavages that would not divide the electorate into equal portions.

Nationalization as a Uniform Response to Political Forces

The dominant conceptualization of nationalization, however, has treated it as a uniform response to political forces. Schattschneider illustrates some of the conceptual ambiguities in this literature by defining nationalization as convergence in levels of support and within a few pages as a uniform political response: "*The universality of political trends is an index of the nationalization of the political system.* Does the same trend appear throughout the country or do conflicting trends appear?" (Schattschneider, 1960, p. 93, emphasis in original).

Schattschneider compared the number of states in which the Republican Party improved on its performance over the last election in the landslide election of 1904 to the number of comparable states in 1952-1956. Since a far greater proportion of states moved in a pro-Republican direction in the later period than in 1904, he concluded that the system was currently more nationalized than it was at the turn of the century (1960, p. 93).

Stokes's more sophisticated analysis also defined nationalization as a uniform political response. In explaining the conceptual basis of his statistical model, he wrote:

> It can be seen intuitively that if the forces moving the electorate were perfectly idiosyncratic to in-

> dividual constituencies, variations of turnout or party strength would show no more than a chance similarity across the constituencies of a state or region or across the whole nation. Detecting no more than chance similarity, the model would attribute all forces on electoral change to the constituency level. But if politics at the state or national level did have common effects across a number of constituencies, turnout or party strength would show parallel movements, and the model would attribute an influence to these higher levels of politics according to the degree of observed similarity. (Stokes, 1967, p. 185)

Stokes separated the variance in his data into three components. One component, which he labeled the national effect, measured the variance accounted for by the uniform movement of congressional districts across a set of congressional elections. A second component—a state effect—measured the variance explained by the uniform movement of congressional districts within each state in those elections. The third component measured the residual variation, variation presumably resulting from constituency level influences on turnout and vote choice.

Thus both Schattschneider and Stokes discussed nationalization in terms of a similar response by all subunits of the electorate in reaction to the political forces in a given year. It seems reasonable to describe a situation in which all subunits are responding with a uniform surge toward (or away from) a particular party as nationalization. However, a collection of subunits characterized by a high degree of uniformity in political responses may be quite dissimilar in their levels of partisan support. That is, although subunits may shift by similar amounts, their normal levels of support may be quite different. In short, these appear to be distinctive dimensions of nationalization.

Nationalization as Response to National Level Stimuli—A Digression

Whereas Stokes's method of analysis quite clearly measured nationalization as a uniform response across subunits, his discussion of nationalization often referred to "national influences" on the vote, such as presidential personality or the performance of the legislative party, which led to Katz's complaint (1973a,b) that Stokes underestimated the amount of nationalization in the American electorate by measuring only the uniform movement of the electorate rather than all the movement—uniform and differential—that occurs in response to national forces.

In reply to Katz, Stokes (1973) wrote that he was well aware of the possible nonuniform effects of national forces. However, he asserted that three of the terms in his model, the covariance terms, were designed to measure such effects, and since these terms approached zero, he had concluded that they were negligible in practice and had therefore concentrated on the analysis of the uniform national response and the constituency effects. Stokes dismissed Katz's findings of a substantial nonuniform national effect on technical grounds.

Our own objection to Katz's findings is somewhat stronger in that we believe his conceptualization of nationalization is quite different from the traditional one, and his measurement of that concept uses inappropriate data. Nevertheless, this exchange points up some further conceptual and operational difficulties in the research on nationalization. First, there is a blurring of two quite distinct ideas—the source of the stimulus, national or local, and the form of the response, uniform or nonuniform. Perhaps Table 1 will illustrate this point.

A uniform response to a national stimulus might be illustrated by the coattail effect that results from the landslide victory of a popular presidential candidate or, conversely, by a nation-

Table 1. Sources of Electoral Stimuli and Forms of Response

	Sources of Stimuli	
	National	Local
Uniform	Presidential Coattails "Throwing the rscals out" 1	Coincidence 4
Nonuniform	Response to energy issue by Snowbelt and Sunbelt 2	"Friends and neighbors" Incumbency 3

wide urge to throw the rascals out after some debacle, like Watergate, in Washington. The response is similar everywhere; the stimulus for it is national-level politics. But, as both Stokes and Katz pointed out, there may also be nonuniform responses to national stimuli, the "downward deflection of national forces," to use Stokes's phrase. The differential response of energy-producing and energy-consuming areas to national deregulation of oil prices might be an example. A third category is the idiosyncratic response of constituencies to local stimuli—the preference for an incumbent regardless of party, or V. O. Key's "friends and neighbors" effect in boosting the vote for a favorite son (Key, 1949). A fourth possibility—a similar response everywhere to local factors—need concern us less, as one would assume such an occurrence to be rare and largely a matter of coincidence.

Once we have raised this set of distinctions, the question becomes: Which of these forms of behavior are nationalization? Katz definitely would include both the first and second; Stokes would also, but would add that the second is an empty cell. It is almost certain, however, that Schattschneider and Sundquist would not include the second cell, since surely one would then have to include as examples of nationalization the sectional responses to the legacy of the Civil War, the epitomy for both Schattschneider and Sundquist of the *absence* of nationalization.

However one answers this conceptual question, it is accompanied by an operational question—how to distinguish among these categories. Katz attempted to distinguish only between national and local stimuli, being content to lump uniform and nonuniform responses together. Stokes, on the other hand, asserted that cells 1, 2, and 3 were all measured by his model: 1, by the national effect, 3 by the constituency effect, and 2 by the covariance terms. We believe that this is mistaken. We are convinced that the constituency effect in Stokes's model measures the differential (or idiosyncratic) responses of the constituency to all forces in the election, be they national, state, or local. In other words, there is no means in analysis of variance-based techniques to distinguish among the *types* of forces which may cause the nonuniform response by the local unit. This is not a problem so long as one is defining nationalization as the uniform response across geographic units since the method does very well in distinguishing uniform response (the national effect) from nonuniform responses (the constituency effect).[4] (For discussion of this point and the application of a related technique, see Flanigan & Zingale, 1974.)

If Stokes's constituency effect measures the idiosyncratic response of local constituencies to all electoral forces, what then of the covariance terms in his model? After considerable pondering of the mathematics of this term as offered in Stokes's original article (1965, p. 84), we have concluded that these terms are properly interpreted as measuring the association between the separate effects. Although this can be helpful in examining common *patterns* of idiosyncratic behavior (see Zingale, 1978, for use of a related analytic technique), it does not appear to be conceptually or operationally an appropriate means to measure the *magnitude* of such responses.[5]

In contrast, Katz's method is an attempt to distinguish the response to national stimuli from the response to local stimuli, disregarding whether the response is uniform or not. As Katz noted (1973a, p. 819), virtually any pattern of subunit change can be compatible with his view of nationalization. Therefore, the primary methodological problem became partitioning the behavior of subunits into two categories: 1) the varying behavior resulting from the subunits' response to local objects, and 2) the behavior resulting from the subunits' responses to national objects, but since their reactions to those objects may differ, the resulting behavior may also vary. Katz's solution was to assume that, if an electorate responded to the same national objects and if all subunits within a given state responded to the same statewide political objects, then the overtime variation in each subunit would be a linear function of the national and statewide electoral forces. To take account of the varying reactions of different subunits to the same forces, he allowed the parameters of this linear function to vary across units. This strikes us as a tenuous procedure. If responses to the same political forces can lead to a great variety of parameter values across subunits, it would seem likely that the functional form of the relationship linking national and state level forces to subunit response could change as well. But if the functional form of the relationship is allowed to vary across subunits, then partitioning subunit variance into that resulting from responses to national political objects and that which reflects responses to local forces becomes an insurmountable task.

Perhaps more fundamentally, response to national or local stimuli would seem to be an individual rather than an aggregate concept since one is considering such factors as individual evaluations of important issues and exposure to information. Thus if nationalization is conceptualized in this fashion, its investigation would better be carried out with individual level data.

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[4] The state effect is, in these terms, the uniform response of districts within a state, but a response that is not uniform across states.

[5] We would like to extend our thanks to John Kemper of the Department of Mathematics, College of St. Thomas, for his help in understanding these terms.

Method and Data

Two overall points emerge from the above review. First, the concept of nationalization has more than one dimension. In order to describe fully the degree to which an electorate is nationalized, we need to measure nationalization both in terms of convergence in the levels of support and in terms of the uniformity of response. Second, existing evidence of the nationalization of the American electorate along each of these dimensions is suspect owing either to various methodological problems in earlier research or the failure of this literature to present the appropriate data. The purpose of the remainder of this article is to fill in some of the gaps in our empirical understanding of the nationalization of the American electorate.

Although we have expressed some reservations about the interpretation of Stokes's method, it remains an extremely ingenious application of a statistical model to a substantive topic and a classic illustration of the appropriate use of aggregate data to investigate an essentially aggregate concept, the behavior of congressional electorates. In this reinvestigation of nationalization, therefore, we plan to use basically the same methodological approach to the problem. We have, however, dropped Stokes's covariance terms from the analysis of variance model since these terms have little substantive interest for us. We have also added an additional term to Stokes's model, allowing us to examine within a common framework both dimensions of nationalization: convergence in levels of support (which Stokes did not include) and uniformity in the movement around such levels.

If we partition the total variation in a set of subunits *I* across a set of elections *J*, we can use the following analysis of variance formula:

Total sum of squares (*SS*) = election *SS*
+ subunit *SS*
+ election-subunit interaction *SS*

or more formally:

$$\sum_{I}^{i=1} \sum_{J}^{j=1} (x_{ij}-\bar{X}_{..})^2 = \sum_{J}^{j=1} (\bar{X}_{.j}-\bar{X}_{..})^2 + \sum_{I}^{i=1} (\bar{X}_{i.}-\bar{X}_{..})^2 + \sum_{I}^{i=1} \sum_{J}^{j=1} (x_{ij}-\bar{X}_{.j}-\bar{X}_{i.}+\bar{X}_{..})^2 \qquad (1)$$

In words, this means that the total variation of the subunit scores around the grand mean of the entire set of data can be partitioned into:

(a) The election sum of squares—the variation attributable to the impact (effect) of differences among the election means;
(b) the subunit sum of squares—the variation attributable to the differences among the subunit means; and
(c) the election-subunit interaction sum of squares—the residual variation due to the impact of a particular election on a particular subunit.[6]

The election sum of squares measures the average variation of all subunits across the set of elections, that is, the portion of the total variation which is uniform across the subunits.[7] In other words, this measures the extent to which subunits respond uniformly to forces in the election and is strictly comparable to the "national effect" in Stokes's analysis. The subunit sum of squares measures the divergence among the average behaviors of the subunits. In other words, the larger the subunit sum of squares, the greater the difference in average level of support among subunits and the lower is the amount of nationalization in terms of the uniformity of levels of support. This aspect of nationalization was not considered by Stokes, and consequently he eliminated this term from his formulation.[8] The subunit sum of squares is similar to the use of standard deviations in Schattschneider's and Sorauf's work, and there is no particular advantage in substituting it here other than the ability to look at it in a common framework with the uniformity in response. The election-subunit interaction term measures the residual variation in subunit behavior after the variation attributable to the uniform effects of particular elections and to the average vote of that subunit have been accounted for. Put differently, it measures the idiosyncratic movement of subunits away from their own expected voting patterns in response to the characteristics of particular elections. This is identical to the constituency effect in Stokes's analysis. Unlike Stokes, we believe that this term picks up all such idiosyncratic movement regardless of the stimuli (national or local) that may have prompted it.

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[6] Since there is only one score per cell in this model, all the variation in the data are captured by these three terms. Consequently there is no error term.

[7] Actually, this measures the *average* movement across all subunits.

[8] Rather than presenting this term as part of the formulas, Stokes took acount of it in the handling of his data. Thus the subunit sum of squares was subtracted at an earlier point in his analysis than in ours, but the effect is the same and the results are strictly comparable.

We can elaborate this model in a fashion similar to Stokes by introducing the nesting of subunits within a higher level subunit. (For reasons discussed below, we have chosen to nest the subunits in regions rather than states.) The formula is:

Total *SS* = election *SS* + region *SS*
+ subunit (within region) *SS*
+ election-region interaction *SS*
+ election subunit (within region) interaction *SS*

More formally, this can be expressed as:

$$\sum_{I}^{i=1} \sum_{J}^{j=1} \sum_{K}^{k=1} (x_{i(k)j}-\bar{X}_{.(.).})^2 = \sum_{J}^{j=1} (\bar{X}_{.(.)j}-\bar{X}_{.(.).})^2$$
$$+ \sum_{K}^{k=1} (\bar{X}_{.(k).}-\bar{X}_{.(.).})^2 + \sum_{I}^{i=1} \sum_{K}^{k=1} (\bar{X}_{i(k).}-\bar{X}_{.(k).})^2$$
$$+ \sum_{J}^{j=1} \sum_{K}^{k=1} (\bar{X}_{.(k)j}-\bar{X}_{.(.).})^2$$
$$+ \sum_{I}^{i=1} \sum_{J}^{j=1} \sum_{K}^{k=1} (x_{i(k)j}-\bar{X}_{.(k).}-\bar{X}_{.(.)j}+\bar{X}_{.(.).})^2 . \quad (2)$$

We can now measure the uniform movement across subunits *within* regions (election-region interaction *SS*) and the divergence of the expected voting patterns of regions (region *SS*), as well as the uniform movement of all subunits (election *SS*) and the idiosyncratic movement of districts within regions (election-subunit (within region) interaction *SS*). The measure of the divergence of subunits becomes less useful as a measure of nationalization of support levels since it now measures the divergence of subunits within regions (the subunit (within region) *SS*).

In implementing this analytic approach, our handling of the data has differed in several ways from that of Stokes. The main changes are: 1) changing the base for calculating percentages to the total vote and examining both parties; 2) changing the units from congressional districts to counties; and 3) changing the nesting from states to regions. We also extended the time period forward and backward, so that we are able to cover elections from 1842 to 1970.

Calculating Percentage over Total Vote. Stokes used the Democratic and Republican votes exclusively in his analysis. In years with little third-party support, e.g., the decade following 1952, the two-party percentages are about the same as those calculated over the total vote. Furthermore, two-party percentaging makes the parties mirror images of one another, so analysis need be done only once. However, the parties are not strictly mirror images, and the intrusion of third parties usually affects one party more than the other. So calculating percentages of the Democratic and Republican vote over the total vote captures the dissimilarities in party voting and creates the possibility that different findings will emerge in examining the two major parties.

Congressional Districts vs. Counties. We imagine that the potential for different results is greatest in our shifting the basis of analysis from congressional districts to counties. Obviously, counties are not the same as congressional districts for this analysis since they do not represent a single congressional constituency. Many counties are subunits within a district, and a few counties have more than one congressional district within their boundaries. There are many more situations where counties are subunits of a congressional district, so the effects of this condition are potentially greater. In such cases, our use of counties allows different parts of a district to demonstrate differential behavior, thus presumably increasing the constituency component. In the rarer case of a large county with several congressional districts within it, we would expect unusually large variation in voting from election to election if the races are sometimes uncontested. Of course, the more districts there are within a county, the less the impact of such irregular contestations in any one district. Still, this is a source of local variation in our analysis which is absent in Stokes's work because he omits a district if any of the five races in a decade is uncontested. Although the absolute level of the constituency effect should be affected by this substitution of counties for districts, it is less clear how this substitution would influence the trend in this effect over time.

What is important to note is that this substitution does not alter either the magnitude nor the dynamic trend of the national effect. In both Stokes's analysis and in ours, the national effect is calculated from the actual nationwide election percentage and is, therefore, unaffected by the choice of subunit.

The greatest concern we had with using counties was their uneven size. Whereas in certain decades there have been discrepancies in sizes of congressional districts that raise problems in democratic theory, as units of analysis they are relatively uniform. Counties, on the other hand, vary tremendously in size, and there is the clear possibility that a large number of small counties, representing relatively few voters, could dominate the analysis.[9] In order to assess how much of a problem this created, we carried out parallel analysis with the counties weighted by size. Generally, weighting depressed the local effects but not by much, and overall the same patterns emerged.

The choices of counties, on the other hand, allows us to maximize the number of cases in the analysis. By using congressional districts, Stokes was forced to omit all states having only one district since there would be no way to differentiate state and constituency effects. In addition, Stokes also dropped a district from the analysis if it were uncontested in any one of the five elections in a decade. The combined impact of these rules is that in no decade do more than two-thirds of the districts enter into the analysis, and in some decades fewer than one-half of the districts are included. Because so many southern districts are periodically uncontested, virtually the entire South is eliminated from the analysis. Since the South has been the most deviant region politically, its elimination would almost certainly increase the estimation of the degree of nationalization. To avoid this problem, we have decided to include all counties for which data are available, regardless of their contestation status. As a result, over 80% of the counties remain in our analysis during each decade examined. The majority of southern counties are included, although it remains true that the bulk of the missing cases are southern. The inclusion of uncontested counties will no doubt inflate the constituency effect in our analysis,[10] but the overtime trend should be less affected.

State vs. Regions. We altered Stokes's model in another way by nesting subunits within regions rather than states.[11] A nested design cannot affect the estimate of national effects, but it could change the variation assigned to the local units. Our expectation was that either states or regions would behave in about the same way in a nested design. We could have tried both states and regions and compared results, but it was one of many minor variations that was never carried out. Conceptually we are marginally more interested in regional effects than state effects since most previous discussions have emphasized regional differences. Basically, however, this constituted a modification that we expected would have little impact on the results.

Like Stokes, we have used sets of five congressional elections, each spanning a decade following the decennial reapportionment, and repeated analysis for each successive set of five elections. Our analysis begins with 1842 and ends in 1970.[12]

Results

Nationalization as Convergence in Levels of Support—Evidence

Figure 3 displays our measure of the degree of convergence in the levels of partisan support across geographical subunits of the United States from 1842 through 1970. The county scores come from the subunit sum of squares in Equation (1) above,[13] whereas the regional scores are based on the region sum of squares in Equation (2). The scores in Figure 3 were derived from an analysis of the Democratic percentage of the congressional vote, but parallel analysis of the Republican vote yielded substantially similar results. Also included in Figure 3 are the scores for the non-southern counties only. Each sum of squares has been normed by taking the square root of the sum and dividing by the number of subunits in the calculation, thereby taking account of the increasing number of counties over the time period and allowing the result to be expressed in easily interpretable percentage terms. (The result is essentially a standard deviation of the average votes of the subunits over the decade.) The higher the score, the greater the dispersion among the mean votes of the subunits and hence the lower the level of this form of nationalization.

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[9] Counties have varied greatly in size over the years. In the nineteenth century counties might increase in population several hundred percent in a decade. Even greater disparities occur between counties. For example, in 1970 in California one county had a population of 484 and another had 7,032,075. For a discussion of the size of units in electoral analysis see Austin, Clubb and Traugott (1981, pp. 81-103).

[10] This greater constituency effect would result from the large and idiosyncratic fluctuation in a party's vote between election years in which the congressional seat was contested and those in which it was not.

[11] The regional categories are: *New England*—Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont; *Middle Atlantic*—Delaware, New Jersey, New York, Pennsylvania; *East North Central*—Illinois, Indiana, Michigan, Ohio, Wisconsin; *West North Central*—Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota; *South*—Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Virginia; *Border*—Kentucky, Maryland, Oklahoma, Tennessee, West Virginia; *Mountain*—Arizona, Colorado, Idaho, Montana, Nevada, New Mexico; *West*—Alaska, California, Hawaii, Oregon, Washington.

[12] The congressional vote data were supplied by the Historical Archive of the Inter-University Consortium for Political and Social Research. The ICPSR bears no responsibility for the analysis or interpretations offered here. The coding of parties is available from ICPSR in study numbers 0019 or 0075.

[13] As noted above, the subunit sum of squares in equation (2) would not be useful for these purposes since it would measure the divergence in levels of support of counties within regions, thereby ignoring the impact of regional differences among counties.

Both the county scores and the regional scores demonstrate the same trend: a steady increase in the dissimilarity in levels of support from the 1840s through the 1920s, a small and temporary decline in the 1930s, a return to higher levels in the 1940s and 1950s, followed by another decline in the 1960s. The unchanging scores for the non-southern counties indicate that the trends in the national data are primarily a result of the changing partisan distinctiveness of the South compared with the rest of the country.

Clearly, the national electorate is less nationalized, in the sense of a convergence in the levels of partisan support today than it was in the 1840s. Part of the difference in our results from Schattschneider (1960), Sorauf (1980), and Sundquist (1973) lies in our use of the congressional vote rather than presidential data. A comparable analysis of the presidential vote, not shown here, shows a steeper decline in dissimilarity (and a greater increase in nationalization) in the 1950s, as one would expect given the departure of the South from the Democratic presidential coalition. One might argue, however, that this represents not the breakdown of a sectional subculture but rather its continuance in the face of the pro-civil rights position of the national Democratic party. (There are, of course, possible causes of this decline that are more supportive of an interpretation of increasing nationalization—greater in- and out-migration, for example.) The difference also lies in our use of a longer time series. Although one can easily see the dip in dissimilarity that was part of the New Deal, the significance attached to this lessens when its temporary nature is made clear and when it can be compared to the much greater convergence existing before the Civil War.

The trends reported in Figure 3 have implications concerning the causes of nationalization. Explanations that emphasize modernization, national development, or innovations in transportation and communication, all of which seemingly imply an inevitable and inexorable movement toward increasing nationalization, are at best only partial explanations. No inexorable movement toward greater nationalization is seen in these trends in convergence in levels of support. Rather, a more political explanation is suggested. The high degree of similarity in levels of support in the 1840s and 1850s and the subsequent decay of this similarity over the remainder of the nineteenth

Figure 3. Dispersion of Average Democratic Congressional Vote by Countries for the Nation and Non-south and by Region, 1842 to 1970 (Low scores indicate greater "nationalization")

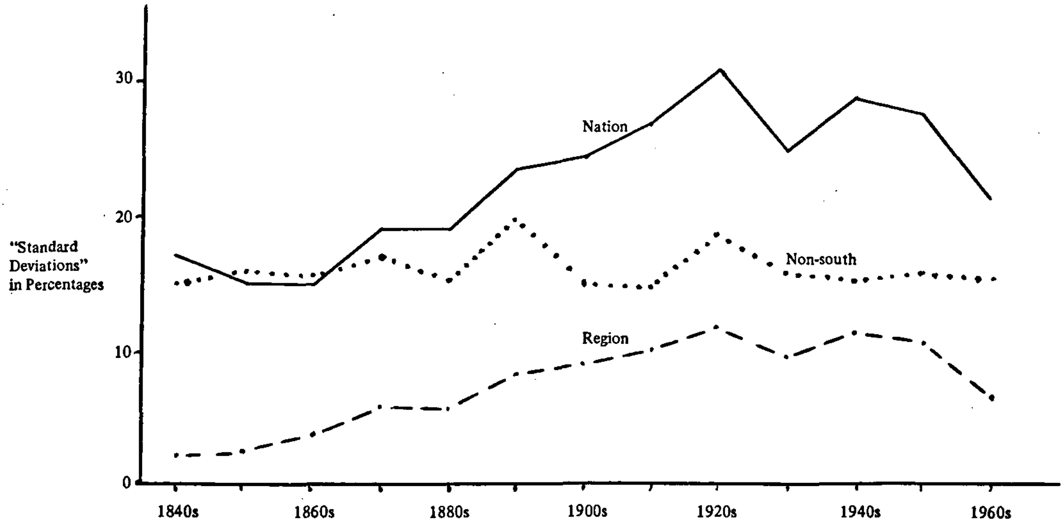
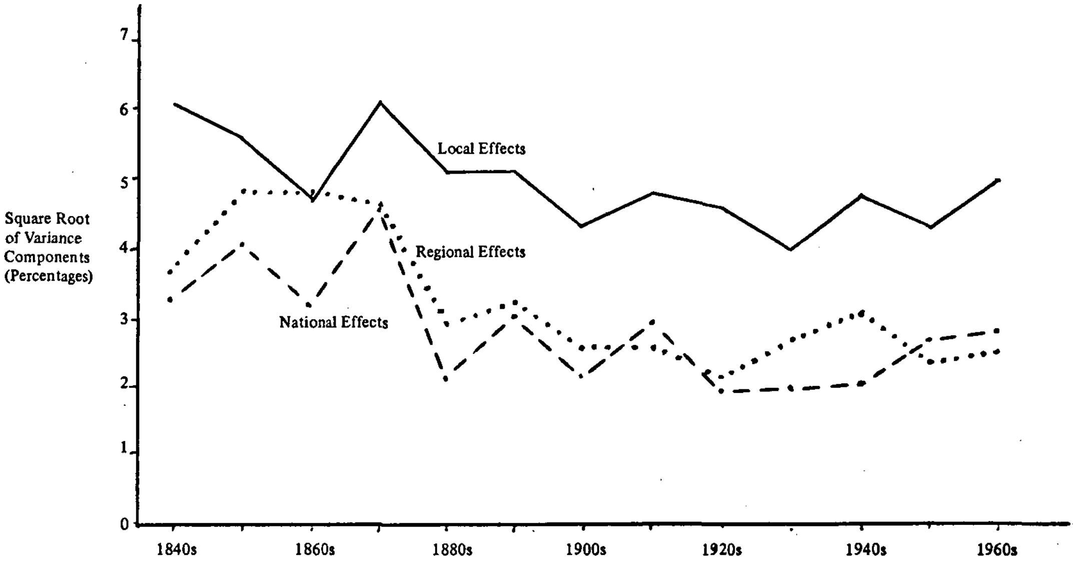


Figure 4. Square Roots of the Variance Components for National, Regional, and Local Effects, 1842 to 1970



century cannot be plausibly explained by suggesting that the South, in terms of its economic and social base, became more distinctive after 1860 than it was before 1860. But it is certainly plausible to suggest that the high nationalization found during the pre-Civil War period may reflect the conscious strategy by the elites of both major parties to avoid candidates and minimize issues that would pit the South against the other parts of the nation. The decline in nationalization after the Civil War can then be explained by a shift in party strategies. Instead of minimizing regionally divisive issues and candidates, party elites of the 1870s and 1880s sought to inflame regional cleavages in the bloody shirt campaigns of that period. (See, for example, Sundquist, 1973, pp. 39-47, 109.)

The convergence in levels of partisan support in congressional voting in the 1950s may reflect, in part, the decreasing social and economic distinctiveness of the South. Political factors surely also play a role. The gradual removal of impediments to black voting in the South, the shifts in ideological position of the Democratic party in the New Deal and in 1948 and 1964, and the development of a conscious "southern strategy" by the Republican party have all doubtless contributed to the convergence in levels of support. This form of nationalization remains, however, considerably less impressive than we might have supposed it would be.

Nationalization as a Uniform Response to Political Forces—Evidence

Before presenting our estimates of the magnitude of this form of nationalization, let us summarize our measures and the terminology we will use to discuss them:

National effect—the election sum of squares; measures the amount of uniform movement across all subunits.

Regional effect—the election-region interaction sum of squares; measures the amount of uniform movement of counties within regions.

Local effect—the election-county (within region) interaction sum of squares; measures the idiosyncratic movement of counties within regions and the nation.

Figure 4 displays these three effects for the Democratic percentage of the congressional vote from 1842 to 1970. The analysis was carried out, as before, on sets of five elections, each set encompassing the decade following reapportionment. Also as before, the square roots of the sums of squares have been taken and the results divided by the appropriate degrees of freedom ($n-1$).

The results found in Figure 4 tell much the same story as our reconstruction of Stokes's data presented in Figure 2 above. The largest component is the local effect, and there is a small, if erratic, downward trend. The national effect, however, far from increasing, declines dramatically in the late nineteenth century and remains low with some fluctuation thereafter.

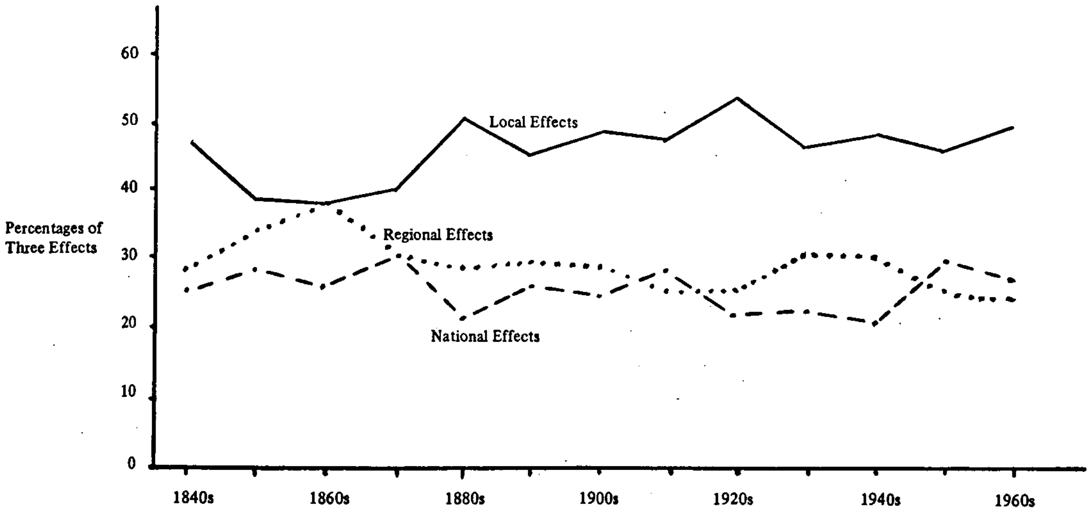
A theoretically more appealing way of looking at the data would take account of the differing amounts of total variation in the data over time. Since we are interested in the relative contributions of the national, regional, and local components, we could be misled by changes in the absolute amounts of these effects if the changes merely reflected increases or decreases in total variation. Figure 5 displays the same results but with the scores expressed as a proportion of the total variation attributable to these three effects, thereby normalizing the results to take account of the differing amounts of total variation in the data over time.[14] Again, no nationalization of the American electorate is uncovered. The only appreciable difference between Figures 4 and 5 is the failure of the constituency effect in Figure 5 to decline over time; rather, it remains fairly stable.

It is important to stress the reasons for the divergence of our results from those of Stokes. Stokes based his conclusion of increasing nationalization in vote choice solely on the decline in the local or constituency effect. The local effect in our analysis also declines, although only slightly and rather erratically. Whatever differences there are here between his and our results may well have come from changing the units of analysis from congressional districts to counties or the consequent recapturing of subunits that were missing in Stokes's analysis or both. These slight differences, however, are not the basis for our conclusion that there has been no increase in nationalization. Instead, our conclusion stems primarily from our examination of the national effect that stays quite steady or, if anything, declines slightly; there is certainly no evidence of increase as would be expected under the nationalization hypothesis. Stokes did not offer any data on the national effect for vote choice; if he had, the results would have been virtually identical to ours, since the calculation of the national effect is not affected by choice of subunit. In other words, our differing conclusions rest on examination of additional and more appropriate data, not on any variations in method that may have been introduced.

Conclusion

The failure to find any increase over the past 100 years in the nationalization of party voting in either a convergence in the levels of partisan support or in the uniformity of response ought to provoke some further rethinking of the concept of political nationalization and its relationship to those societal trends in communication and transportation that appear so obvious. We wish to em-

Figure 5. Normalized Variance Components for National, Regional, and Local Effects, 1842 to 1970



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[14] Stokes presented normalized results in his 1965 article, comparing the contributions of his components to variation in turnout and in partisan vote. He did not use normalized figures in his overtime analysis, presumably because he looked only at the constituency effect and did not measure either the national or state effects.

phasize that the data presented here in no way undermine the observation that *turnout* is highly nationalized, becoming so over the course of the last hundred years. Rather, our data show that the casual extension of this observation to include partisan voting is not warranted.

Even in the area of turnout, however, a reservation must be expressed. If non-normalized figures are used, as Stokes did in his overtime analysis, the national effect on turnout will increase as the variation in turnout among elections increases. When dealing with congressional elections, this means that the national effect will get larger as the difference in turnout between presidential and off-year elections increases. This is, in effect, a measure of "drop-off," to use Burnham's (1965) terminology. Although the increase in drop-off is an interesting enough phenomenon, we wonder whether it is exactly what we have come to think of as nationalization of the vote. Put another way, would we want to interpret a decline in drop-off as a decline in nationalization? The best test for the existence of nationalization in turnout—a test we will leave for later or for other analysts—may be to separate presidential year elections from congressional year elections and analyze the uniformity of turnout responses within each category.

Despite this reservation, we have no difficulty in accepting Stokes's original conclusion (1965, p. 76) that "the great contests for the presidency have extraordinary importance in getting people to the polls but they do not dominate what the people do there in voting for Congress." It remains surprising that "the great contests for the presidency" appear to generate no more of a uniform response on the part of the electorate in the age of television than they did at the turn of the century.

The key to understanding this result may lie in the distinction made earlier between the source of the stimulus and the form of the response. The plausibility of the nationalization argument rests on television, along with rapid transportation and other forms of communication, creating a national political environment and bringing the same events and issues to the attention of people (and voters) everywhere—in short, providing a common set of national stimuli. But then the argument makes something of a leap: not only does television provide a common set of stimuli, it also provokes a common set of responses. We hear (and probably make) this argument in a variety of circumstances: that television is breaking down regional political cultures, replacing them with a single national culture; that regional dialects and speech patterns are not long for this world; that regional racial prejudices will be overcome by integration openly practiced on TV.

But counterthemes have also been heard, perhaps most notably that the racial unrest of the 1960s was fueled by television literally bringing home to the poor and black the advantages enjoyed by affluent white America. Thus a common set of national stimuli is no guarantee of a uniform national response.

Nationalization has been most often defined as increasing uniformity in response or in level of partisan support across all areas of the nation. This has never meant homogeneity in the behavior of all individuals but rather similarity in the proportions of differently behaving individuals in all geographic subunits. The implicit assumption is that modernization will increase the similarity of this mix across the nation and thus increase the uniformity in level of support and in political responses. Perhaps in this we have been influenced by the Marxist view of industrial development—in an advanced industrial society, the most important cleavages will be class or economic cleavages and all moderately large geographic subunits will have roughly similar mixes of middle and working class people.

However, to the extent that the salient political cleavages in a society follow, rather than cut across, geographic divisions, a national political environment and national media focusing attention on the same issues everywhere may serve to accomplish the opposite of nationalization, that is, to stimulate dissimilar behavior on the part of geographic units. As the issues pit Sun Belt against Snow Belt, energy-producing states against energy-consuming states, the Bible Belt against the "godless" cities, the developers in the West against the conservationists from the East, we should not be surprised if national attention to and knowledge of these issues conveyed by television and all the other accoutrements of advanced technology bring no increase and perhaps even a decline in the nationalization of the American electorate.

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In Search of the Center of European Party Systems

HANS DAALDER
University of Leyden

Duverger's famous dictum *that "the center does not exist in politics" is the starting point of an inquiry into the possible meanings of the concept of a center, and of center parties, in European party systems. This article consists of six sections; the first five sections deal with the center as a pivot in voting, the center in traditional left-right distributions, the notion of a center in multidimensional party spaces, the center in terms of mechanics, applying rather different metaphors of scales-in-balance and of centripetal versus centrifugal forces, and the center analyzed in terms of social cleavages. Following this search for conceptual meaning, in which the writings of Duverger and Sartori receive particular attention, the record of European party systems is examined to see whether the presence or absence of center parties in party systems can contribute to a realistic classification of European party systems.*

This article has its origin in a curious anomaly. Writings about European party systems abound with references to the Left. They also give considerable attention to a less easily defined Right. Yet, there is practically no systematic treatment of the center or of center parties. Admittedly, the term *center* is widely, if loosely, used in both journalistic comment and in academic descriptions of the functioning of party systems in individual countries. Nevertheless, European party systems are rarely viewed from a center perspective. On the contrary, the idea of a center tends to find little favor with scholars. More than a quarter of a century ago, Duverger argued in a famous paragraph of his book, *Political Parties:*

> The center does not exist in politics; there may well be a Center party but there is no center tendency, no center doctrine. The term "center" is applied to the geometrical spot at which the moderates of opposed tendencies meet: moderates of the Right and moderates of the Left. Every Center is divided against itself and remains separated into two halves, Left-Center and Right-Center. For the Center is nothing more than the artificial grouping of the right wing of the Left and the left wing of the Right. The fate of the Center is to be torn asunder, buffeted and annihilated: torn asunder when one of its halves votes Right and the other Left, buffeted when it votes as a group first Right then Left, annihilated when it abstains from voting. The dream of the Center is to achieve a synthesis of contradictory aspirations; but synthesis is a power only of the mind. Action involves choice and politics involve action. . . . There are no true Centers, only superimposed dualisms. (Duverger, 1964, p. 215).

Giovanni Sartori has written of center parties in hardly more positive terms. He greatly values what he terms *centripetal drives* in party systems —i.e., a competition of parties for votes *at* the center. But for precisely that reason he takes a negative stance toward the "physical occupation of the center" by one or more center parties, which he singles out as one of the distinctive features of systems of "polarized pluralism." For this implies that "the central area of the political system is *out of competition*. . . . In other terms, the very existence of a center party (or parties) discourages centrality, i.e., the centripetal drives of the political system. And the centripetal drives are precisely the moderating drives. This is why this type is center-fleeing, or centrifugal, and thereby conducive to immoderate or extremist politics" (Sartori, 1976, pp. 134-135 and secs. 6.1 and 10.4; for earlier versions, see Sartori, 1966, 1970; for a very useful and more complete anthology of his contributions in Italian, arranged in chronological order, see Sartori, 1982).

Such critical attitudes toward center parties

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The idea to study the possible use of the "center" concept in the analysis of European party systems originated in a seminar on *Recent Changes in European Party Systems* which I directed at the European University Institute in Florence in 1978-1979. A number of seminar participants formed a small working group in which a number of preliminary papers were presented. Members were Stefano Bartolini, Karl Dittrich, Maurizio Ferrera, Lars Johansen, Peter Mair, Francois Petry, and Pier Vincenzo Uleri. Gunnar Sjöblom also participated in some of the group meetings.

I am most grateful to Giovanni Sartori and Galen A. Irwin for their critical dissection of an earlier draft, and to Peter Mair for editorial help.

stand in marked contrast to the rhetoric of many participants and observers of European party systems. Clearly, the idea of center parties has been regarded as an attractive one by politicians, who in recent decades have regarded the very word *center* as a positive label for their parties. The term has been adopted by former agrarian parties in Scandinavian party systems, by new parties in various reemerging democracies of Southern Europe, and among would-be reformers of party systems in a Giscardist France or a Jenkins-type Britain. *N'en déplaise* Duverger, terms like *centripetal competition, centrism* and *center* generally carry positive overtones. But such sentiments are rarely given specific meaning or explanation.

Academic writings in this area are neither extensive nor well focused. Scanning the literature (e.g., on party systems, political cleavages, or coalition theories), one finds few explicit references to the concept of a center and its use in the study of party systems. In addition to Duverger and Sartori, only Downs (1957) seems to have made the idea of a center a key variable in his writings. Impicitly, some attention is given to the notion by authors who have studied the formation of European cabinet coalitions, based on an assumed location of parties along one or more dominant dimensions (e.g., De Swaan, 1973; Dodd, 1976; Taylor & Laver, 1973). The writings of formal theorists shed some indirect light on the subject, notably because they specify the conditions that must be met if one is logically to speak of a center in a multi-actor game. As a whole, however, the literature offers few cumulative lessons, possibly owing to at least two factors.

First, the term *center* forms part of at least three rather different kinds of metaphors. Thus, some authors reason in *locational* terms, holding that a particular individual or party occupies a center position in a given political space. Others reason in metaphors taken from *mechanics:* e.g., certain parties hold the balance in a political system, or the interaction of parties shows the operation of centripetal or centrifugal forces. Yet other writers focus in particular on specific *kinds of cleavages,* which may be inherently dualistic in nature (as Duverger would argue as a matter of course), or may allow more variegated positions.

A second confusion results from failure to specify the unit of analysis. For some, the center is a theoretical point, to which parties may be near or distant. For others, the center is an actual actor, whether a pivotal deputy in an actual parliamentary vote or one or more parties in a given party system. But in treating a party as an actor, new ambiguities may arise, depending upon whether or not a party can in fact be treated as a *unitary actor,* or whether it should be analyzed in terms of possible internal divisions (e.g., within the parliamentary group or between this group, party activists outside parliament, and voters for a party).

The importance of these distinctions should become clearer in the course of the following exploration, which will move from the spatial concept of center (sections 1-3), to approaches reasoning in mechanistic terms (section 4), to analyses in terms of actual social cleavages (section 5).

The Center in Voting

The Median Member or the Median Party

It is clear from the passage quoted above that Duverger bases his view of the nonexistence of the center on the need for a voting body (such as a parliament) to say either yes or no to a particular proposal. Hence, he refers to the center as a mere "geometrical spot," which separates the proponents and opponents of a particular decision. As the word suggests, such a point is most easily envisaged as a point on a line on which all members of a voting body are arranged. The center, then, equals the position of the "median member"—a conclusion substantiated in formal theory by Black (1958). Certain problems emerge from this proposition, however. How does one know who the median member is? If one is to locate the median member of a voting body, members must be somehow arranged in a logical order, which is tantamount to saying that there must be a common underlying dimension, a point that will be taken up shortly. But how can one substantiate this dimension, except in a concrete vote, and only *a posteriori*?

If one analyzes decisions of a voting body in terms of parties as actors rather than in terms of individuals, what is the relationship between this median member and the party in which he is included? Can one describe the latter party logically as the center party, or should one instead speak of a party containing the median or central member?

If one wishes to denote the party containing the central or median member as the central party, then difficulties arise when one tries to apply the concept to a two-party system. For in such a system the majority party would become the center party by definition. In the case of alternating majority parties, this would imply alternating center parties. If parties do not alternate, one is in the presence of a *predominant* party. Although such a party must be of central importance to a system of this kind, does one gain any insight by also calling such a party a center party?

In systems with more than two parties, two conditions may exist. On the one hand, although there are more than two parties in the system, one party may still have the majority. Hence, it occupies the center point by definition. In this case, nothing changes from the previous situation, except that now there are more noncentral parties. On the other hand, in cases where no party has a majority, one must have some way of establishing which party contains the median member, which again raises the problem of an underlying dimension, which will be treated in the next two sections.

Finally, one might attempt to shift the argument from the median member to the median party, i.e., the party that finds itself hemmed in by as many parties on one side as on the other. But such a shift presupposes that it is the number of parties which is important rather than their relative sizes. This proposition clearly leads to ridiculous conclusions where a party finds two very small actors on one side and two very much larger ones on the other. The party that would find itself in such a middle would in any case not be the party containing the center, as defined by the median member.

In summary: to make the center in a voting body identical with the median member or median party and to apply these concepts to party systems raises at least two major problems. First, one must find some way in which to rank-order members or parties or both in relation to one another. Second, one must somehow take into account the different sizes of the parties in a particular system (for an example, see Budge & Herman, 1978).

The Problem of Successive Votes

Even though one may solve the problem of locating the median member in a particular vote, no solution is yet given for locating the center in a succession of votes. Although each vote may logically have its median member, there is no reason whatsoever that the same member will be the central member for each vote, unless all votes represent only one common dimension.

This dilemma is typically solved in escapist ways. One approach focuses on one crucial vote only, e.g., on the vote necessary to confirm a new cabinet in office. All other votes are then made subordinate to the needs of the cabinet coalition thus formed. Another approach seeks to arrive at a stable positioning of individual members (and their parties) by shifting from voting to self-location, e.g., by asking members to place themselves on a left-right scale. A third approach seeks to analyze a selected number of actual votes in order to determine whether members (or parties) fall in place around so-called pivotal actors who are thought to be of central importance. In all such cases, successive votes are really subordinated to presumed dominant alignments. If in practice more than one dimension is found, that one specific dimension is thought to be the decisive one. Hence one can maintain the concept of a median member (or center), even though that member is not central in all actual votes.

The Shifting of the Center

The assumption that the median member is the center becomes particularly questionable in the following cases.

1) In a well-known study in the mid-1960s, Giovanni Sartori arranged the Italian parties along one line and noted that different "centers of gravity of the parliamentary majorities" existed according to the issues faced by the Italian parliament, e.g., an economic majority, a political majority "committed to the defense of the system, that would unite on basic constitutional issues, and that opposes any shift in international alliances," and a religious majority (Sartori, 1966, pp. 160-165). Sartori did not pursue this idea and still speaks in the same article of the center in the singular rather than in the plural. But he then adds: "a center is relative to its left and right wings, it will follow in the long run the general trend of the polity" (p. 164) and "the center will move if the balance between its left and its right should shift" (p. 165).

2) The location of the effective center will change if the votes of certain parties at the extremes are regarded as illegitimate, and conversely if the area of legitimate space expands, situations that have characterized the French Fourth Republic and postwar Italy.

3) One might argue in a similar fashion that the formation of a relatively permanent political coalition logically implies a shift of the pivotal member away from the median of parliament to the median of the coalition. But this argument is in turn subject to criticism—e.g., by those who see the key to power within a specific coalition to be in the hands not of its median member or party, but of those supporters of a coalition who provide the last necessary votes and who conceivably could also shift their votes to an alternative winning coalition, which again suggests that the idea of the median member (assuming such a member can be located) is of little value in the analysis of

successive votes and potentially changing coalitions.

The Center in Traditional Left-Right Assumptions

The Influence of Geometric Thinking

The idea of arranging parliamentary deputies or parties on a left-right dimension has an immediate and intuitive appeal. It is general practice in expositions of single-country studies, and it is widely acceptable to political elites and citizens alike, as is amply proved by the success of surveys containing questions in which respondents must place themselves or parties on linear scales (e.g., Inglehart & Klingemann, 1976; Sani & Sartori, 1983; Sidjanski, 1979).

At a minimum, however, one must be wary that a left-right imagery does not take an uncritical upper hand because of the sheer ease of geometric understanding. The terms *left* and *right* had their origin in the seating arrangements of the French Convention. Just as seats gained a symbolic importance, so the very terminology has tended to acquire a bias towards the left which unavoidably affects future spatial locations.

Undoubtedly, the idea of a linear left-right dimension is highly evocative, as it permits not only notions such as the extremes or the center, but also intermediate ideas like center-left or center-right. But the same notion can also be deceptive, if it is assumed that apparently similar distances at any part of the scale are also similar in political content and affect.

Finally, the left-right image is also compatible with that other intuitively attractive geometric representation: the bell-shaped curve of a normal distribution, which is easily superimposed on a left-right scale. Since in a normal distribution the large majority is clustered near the median, the idea that there is a logical force working for moderation in electoral competition may be uncritically adopted as a basis for further analysis. In that case, no attention needs to be paid to problems of real-life situations, where the mean, median, and mode are not coterminous, let alone to the critical importance of there being only one or more modes in a given distribution of political preferences.

Because the idea of a center depends on the possibility of locating parties on a left-right scale, the manner of obtaining such placements is of critical importance. There are at least three ways in which scholars have sought to solve this problem: asking respondents thought to be representative of a party to place themselves, their own party, and other parties on an imposed left-right scale; scaling or factor-analyzing responses to other survey questions so as to trace underlying dimensions; and analyzing of actual behavior.[1]

Individual Self-Locations and Placement of Parties on an Imposed Left-Right Scale

Subjective placements on imposed left-right scales meet with certain problems such as nonresponses (generally found less frequently among elites than in mass responses); faulty responses (the authors of the Eight-Nation study found by follow-up questions that as many as 1 out of 5 voters might have a totally irrelevant notion of the scale on which they were prepared to place themselves (Barnes, Kaase et al., 1979); a tendency with many respondents to play safe and choose a center location or to eschew the more extreme positions; differing interpretations about the use of the scale; assumptions concerning the interval nature of the scale, necessary to execute arithmetical operations; the problem of scale length (e.g., the finding that the use of 7, 9, or 10-point scales makes for material differences. See the excellent treatment in Irwin & Wiebrens, 1979, to which the findings of Van der Eijk & Niemöller, 1983, should be compared).

Assuming that these problems are not of major importance, scholars have used the responses of both mass and elite surveys to obtain an "average" placement of a party. Thus, Sani and Sartori (1983) take averaged responses from the mass surveys in the eight-nation study to obtain the locations of parties, which they then use in a large-scale systematic analysis that regards the ideological distance existing in party systems as a key variable. They then use these data for a cross-national comparison of the extent of ideological polarization of party systems. Other writers have preferred to rely on elite data as providing more authoritative responses. The problem of finding the correct respondents for the desired location of parties is an important one whenever substantial differences are found, for example between the self-location of a party leader, individual members of the parliamentary party, the leaders outside parliament, and the party voters, not to speak about the assigned locations of one party by leaders or voters of other parties.

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[1] In the following pages I rely heavily on experience with different types of analysis of data obtained in two oral surveys held with members of the Dutch Lower House in 1968 and 1972. The 1972 questionnaire formed part of a study of representation initiated by Philip Stouthard and Warren Miller in which a cross-section of the population was also surveyed. For reports on the data see Daalder and Rusk, 1972; Daalder and Van de Geer, 1977; De Leeuw, 1973; Hazewindus, 1971; Hazewindus and Mokken, 1972; Irwin and Thomassen, 1975; Irwin and Wiebrens, 1979; Thomassen, 1976; Van de Geer and De Man, 1974.

The problem becomes particularly crucial if certain systematic biases have an influence on the data. Thus, it has been a common finding that party activists take up more extreme ideological positions than either party leaders or party voters (e.g., May, 1973). Such a crucial finding should not be hidden behind the analytical need to locate a single placement of a party. Survey responses suggest a consistent tendency in some countries for legislators to place themselves to the left of the location they attribute to both their fellow deputies and their voters (e.g., Van de Geer & de Man, 1974).

Scaling Other Survey Questions

In a number of countries (e.g., the United States, Sweden, Switzerland, and the Netherlands), studies have been undertaken to compare the responses of legislators and voters to the same issue questions. The findings of the 1972 Dutch study may illustrate some of the problems that are likely to come up if one seeks to use these data to trace the existence or nonexistence of one dominant left-right dimension. It contained the following items relevant to attempts to place parties on some form of underlying dimension (in addition to locations on a self-imposed left-right scale): preference rank orderings, sympathy ratings for particular parties, and placements of self, one's own party, party voters, and the other major parties on seven issues (each issue represented by 7- or 9-point interval scales). The following conclusions were drawn.

Strong indications were indeed found for the existence of a left-right scale. However, the best result was not one straight line, but rather a horseshoe curve, with the extremes on the left and right approaching one another at either end, and the major system parties showing substantial differentiations in the lower bend of the figure. The Dutch psychologist Van de Geer explains these findings in terms of a general law of perception which allows for much greater discriminatory perceptions in intermediate zones as compared to extremes. Does this finding tally with Sartori's use of the concept of space elasticity and his particular conclusion?

> A short space does not allow, or does not facilitate, the perception of a center: It has, so to speak, no room for it. A short space is defined simply by its ends—left and right. A third point of reference—*the central point*—becomes meaningful and perceivable only as the space extends, and particularly when the ends of the space are perceived as being two poles apart (Sartori, 1976, p. 347).

However, the placement of parties that results from the scaling of voter responses showed substantial differences from the results obtained from interviews with legislators. This is true both of the general space, and more important, also on the location of parties by voters and deputies on particular issues. Hence, the question arises again: whose placement is decisive, and what issues are the significant ones?

Also, some issues clearly broke out of a left-right dimension. This was particularly true of abortion, defense, and law-and-order issues at the elite level, and of abortion and development aid at the mass level. In other words, although a left-right dimension was consistently found (if one reads along a horseshoe curve), and although the placement of parties along this dimension tended to substantiate stereotyped notions on the assumed location of parties in relation to one another on particular issue scales, the dimension did not fit important issue areas that the parties must deal with. Said differently, the left-right dimension was sufficiently robust to allow the singling out of some parties as having a center location. But on certain issues, neither the scale nor the location held. Clearly, presumed center parties are not center parties on all issues!

Finally, a separate analysis of deputies from the major party groupings showed that not all groupings saw the space obtained from scaling all responses by all deputies in a similar manner. In particular, members of parties that others assigned to a location on the right tended themselves to see the space as one in which their location was much more in the middle. In other words, some parties' centers need not be other parties' centers!

The Analysis of Actual Behavior

At least three kinds of actual behavior lend themselves to a test for left-right (and hence center) placement of parties: participation in government coalitions, parliamentary voting, and official party pronouncement such as campaign statements or party platforms.

Analyses of coalition behavior have increasingly considered the role of ideology. The most successful explanations have been what are called "minimal range" or "minimum connected winning" theories which hold that coalitions are formed containing adjacent parties in sufficient number as to ensure a majority (e.g., De Swaan, 1973; Taylor & Laver, 1973). However, the placement by De Swaan, Dodd (1976) and others of parties in relation to one another is based on the judgment of "authoritative observers" of the politics of a particular country. As these judgments may well be based mainly on their knowledge of actual coalition behavior, there is a danger that coalitions are explained on the basis of knowledge of coalition behavior: this clearly smacks of a tautology. Notions like *range, adjacent,* and *connected* or *closed coalitions* meet with the problem of whether one dimension is sufficient to account for actual coalition behavior. In constructing a policy scale defined mainly in socioeconomic terms, De Swaan must take considerable liberties in forcing parties on to such a scale. (One should note in particular his placing of religious parties, and his reinterpretation of the right part of the scale so as to accommodate authoritarian and national-socialist movements, as well as anticollectivist groupings, at one and the same end of a continuum.) Finally, the placement of parties on a scale is generally static over time. Hence, the data throw little light on dynamic movements that may affect the center as much as either end of the scale.

Data on the way parties line up in parliamentary voting have a considerable advantage over coalition data in any attempt to place parties on a scale. As Pedersen, Damgaard, and Nannestad Olsen (1971) have convincingly shown, analysis of such data allows inspection of the movement of parties over time. It also makes it possible to trace the actual behavior of parties in certain issue areas as distinct from others. Parliamentary voting is, of course, heavily influenced by whether or not parties participate in a government coalition. But this problem may to some degree be overcome, particularly in cases where there is some degree of autonomy between the governmental and the parliamentary arenas.

A third indicator may be found in a systematic study of party manifestoes along the lines of the study on British manifestoes by David Robertson (1976). An international project in this area has been initiated by Robertson and Ian Budge. One must await publication of the results before one can conclude whether or not this type of study makes it easier to arrange parties on some more durable left-right scale.

Conclusions on the Left-Right Scale

Whenever a robust left-right scale can be established, the assumption that there is a center at some intermediate point or area offers at least some promise. However, the foregoing paragraphs have raised certain difficulties: the actual placement of parties on such a scale may differ according to the category of respondents interviewed, the kind of data and analysis techniques used, the actual issues at stake, or the time studied. Although left-right perceptions are strongly present, there is not necessarily an immediate relation between perception and behavior. More important, for certain actors, and in certain issue areas, the left-right dimension does not hold. Often political realities prove stubbornly resistant to attempts to reduce a multidimensional universe to a unidimensional ordering.

Does a Center Exist in a Multidimensional Space?

Geometric Representations

Clearly, the idea of a center in a multidimensional world is intuitively a great deal more difficult to grasp than in a linear representation. Even in a simple two-dimensional representation of a triangle (see Figure 1a), with three parties B, C, and D at the points, one could associate the idea of a center with four positions: the midpoints between BC, CD, and BD, and the point A equidistant from B, C, and D. Even easier to grasp is the idea of a circle (Figure 1b)—or in a three-dimensional representation, a sphere—on which all parties are placed. In such a representation there is indeed only one center, but this emerges at the expense of any meaningful insight into dimensions. Another possible location is the point of intersection between two dimensions: at this point one actor (A) would be placed centrally on two dimensions, unlike other actors who might be central on one dimension, but not on the other (B and C, respectively)—see Figure 1c. Such spatial locations are less suggestive, however, than locations on a simple line. (But see Budge & Farlie, 1977, chap. 9, for an imaginative use of the concept of central "zones" between parties.)

Duverger: The Center as a Result of Superimposed Dualisms

As we noted above, Maurice Duverger derives the existence of a center or centers from "superimposed dualisms." Figure 2 reproduces his well-known circle of overlapping cleavages. The figure indeed suggests one possible explanation of multipartism. But such an explanation does not say anything about a center, unless one projects all parties onto one dimension. Thus, the Socialists, the Progressive Christians, the Radicals, and the MRP become center parties if one takes the Communists and the RPF as the end points of the decisive dimension. This is saying little else than that a center emerges if one reduces a multidimensional space to a unidimensional ordering. Such apparently is the strength of the left-right imagery, even for an author who sets out deliberately to

Figure 1. Three Possible Geometric Representations of Parties in a Multidimensional Space

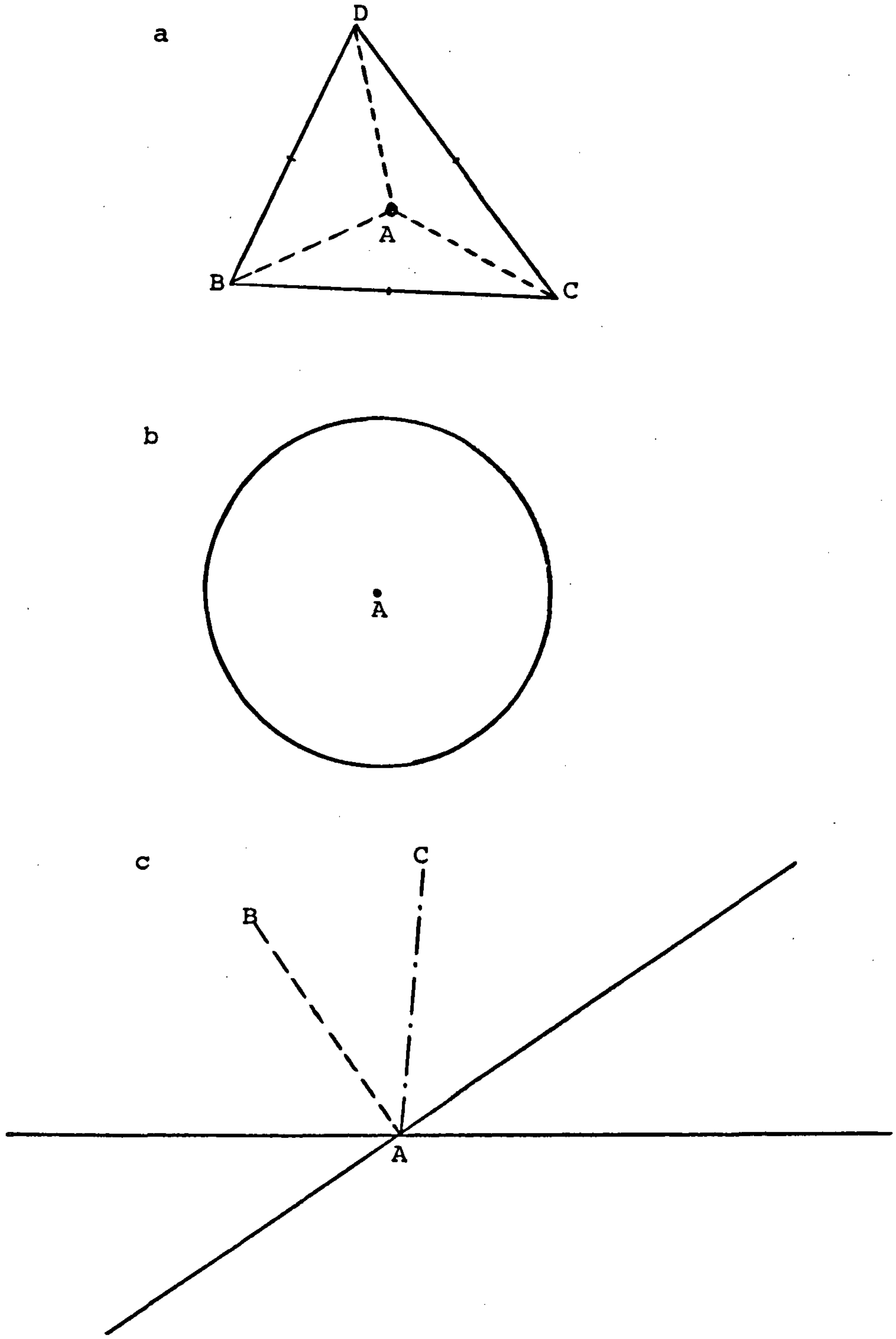
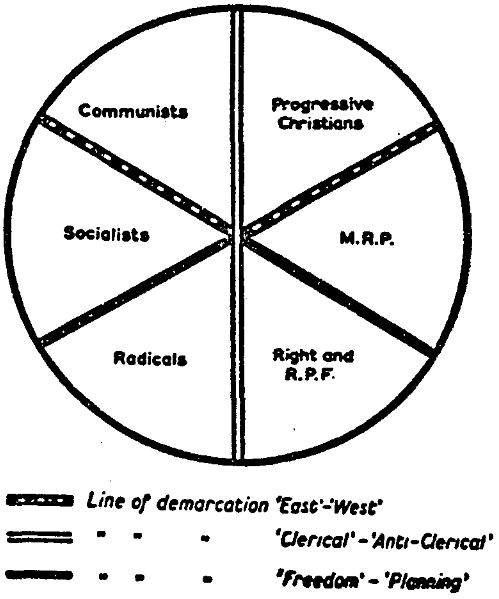


Figure 2. Overlapping of Cleavages in France


Communists
Progressive Christians
Socialists
M.R.P.
Radicals
Right and R.P.F.
Line of demarcation 'East'-'West'
" " " 'Clerical'-'Anti-Clerical'
" " " 'Freedom'-'Planning'

Source: Duverger, 1964, p. 232.

analyze the simultaneous existence of more cleavage lines!

One should add that on closer inspection Duverger does not offer just one theory about the ("non-existing") center, but many. Thus one may distinguish at least the following three:

1) the assumption that all real political choices are inherently dualistic. Hence, a center can only be defined as a pale reflection of the real forces at the poles and is inevitably divided between the opposing attraction exercised from the two poles, of *one and the same* dimension;

2) the proposition that a center is the result of *projecting parties on one overriding axis* where others form the defining poles (e.g., Communists vis-à-vis the Right);

3) the view—still to be treated—that center parties are really the residue of past history, as once-progressive forces are being overtaken by new forces on the Left (Duverger's *sinistrisme*).

Sartori: A Unidimensional Space of Competition in a Multidimensional Political Universe?

No writer has grappled with the dimensionality problem more gingerly and persistently than Giovanni Sartori (see in particular Sartori, 1966; 1976, chap. 10; and Sani & Sartori, 1983). Sartori has been aware from the outset of the multidimensionality of issues and identifications in contemporary politics, and he has been very conscious of the indeterminacy of meaning, and the strong biases, of left-right imagery. He has strongly criticized authors who rely on a socioeconomic interpretation of a left-right scale in the face of contrary evidence and has suggested instead that priority should be given to a constitutional-political interpretation. He has warned against the facile assumption that similar intervals in a spatial representation also reflect the same actual political distances. A major point in all his writings has been that there are indeed major ideological divides which separate system parties from nonsystem parties. He therefore uses terms like *different spacings* and *disjointed spaces.* Yet, again and again, he has attempted to reduce the multidimensionality of modern politics to a mainly unidimensional interpretation, at least as far as the dynamics of interparty competition are concerned. To do so he follows a number of roads.

1) His primary interest in Volume 1 of *Parties and Party Systems* is to push for an interpretation of the interaction of parties as independent rather than dependent variables. Hence his strong emphasis on the role of the number of parties, and the mechanical effects of their interaction. From it he deduces a number of properties vital for his interpretation, including his notion of space elasticity (the larger the number of parties, the wider the space), the appearance of one or more parties occupying a center position, the displacement of centripetal competition in a system with bipolar mechanics by centrifugal competition in a multipolar one, and eventually as a result of a policy of out-bidding and out-flanking by irresponsible extremist parties, the likelihood of the growth of antisystem parties in a polarized system bound for destruction.

2) Following Converse, Sartori suggests that in given systems a variety of dimensions may coexist, but that these may be of unequal "length." In that case, a process of foreshortening may effectively drive a party on the shorter dimension onto the longer—more salient and dominant—dimension. (See also Sjöblom, 1968, pp. 170 ff.)

3) As we saw, Sani and Sartori (1983) present extensive evidence that on a self-imposed left-right scale voters place themselves much wider apart in some European countries than in others. The resulting degree of polarization (for which they provide a variety of measures) shows a gratifying correspondence with Sartori's distinction between "polarized" and "moderate" multiparty systems. In the same article, these two authors also relate self-locations on a left-right scale to responses to a number of important issue and attitude questions and often find a substantial relationship.

4) Sartori also continues to grapple with the problems, most clearly posed by the so-called segmented societies, in which there clearly is a world of multidimensional identifications. He submits that these identifications cannot by definition enter strongly into the world of electoral competition because that would effectively suppose the presence of at least two parties competing along one and the same ideological dimension for such voters as are available to either contestant. Instead, Sartori submits, the major dimension for electoral competition in most European countries today is left-right competition. Parties not on that dimension can only engage in defensive competition, not in expansive strategies. They also do not really affect the decisive interplay of the party system, unless they engage in the struggle along the general and dominant dimension. In this way, Sartori explains why in certain party systems (e.g., the consociational democracies characterized by segmented societies), there are five parties or more, which yet do not show a tendency toward increasing polarization. There may be numerous parties with various identifications, but for the effective dimension of competition, such systems represent cases of party crowding rather than of strong polarization.

I have summarized Sartori's arguments rather extensively because his distinction between "domains and identification" (which are often multidimensional) and a "space of electoral competition" (which is much more likely to be unidimensional) could solve the problem of whether a center can exist in a multidimensional party landscape. If the space of competition is effectively unidimensional, a center emerges as a result of the interplay of numerical factors in the way Sartori describes.

I offer the following comments.

At first reading, Sartori's reduction of a multidimensional world of identifications to a unidimensional space of competition shows some resemblance to Duverger's reasoning. In Duverger's case center parties emerge because they are forced to locate themselves between the end points of a continuum which is apparently thought to be the dominant one. Duverger does not make clear, however, why parties not on the dominant dimension should end up near "the" center of a party system, or even at some point in between the extreme poles. Is this because parties formed on the basis of another dimension are likely to harbor rather heterogeneous elements, if looked at from the standpoint of the overriding dimension, which in the analogy of the "crosscutting" proposition forces such parties to choose a somewhat interdeterminate and hence "centrist" position? Whenever different dimensions exist, there is no *logical* reason to hold that they will intersect anywhere near the center of any other relevant dimension. The manner of intersection is a matter for empirical enquiry and should take into account that a particular point of intersection need not be a stable one. Much will depend on such varying factors as the angle of crosscutting, the compatibility of the dimensions, the bargainability of issues, and *ad hoc* or lasting coalitions (which may lessen the salience of particular dimensions).

The actual location of parties on a dimension (which is not the dimension on which they themselves are based) proves in practice to be a task of considerable complexity. As we saw, Abram de Swaan went through the exercise of trying to fit all European parties in the countries he covers on one overriding dimension, which, following Downs, but unlike Sartori, he defines in socioeconomic terms. He inevitably met with great difficulties which he could only solve by a combination of arbitrary choices and a redefinition of the right end of the scale, so as to accommodate clerical, extreme liberal, nationalist, authoritarian, and fascist parties indiscriminately, rather a case of saving the hypothesis of one unidimensional analysis!

Sartori's view on the existence of one overriding "space of electoral competition" is argued in terms of logic rather than based on strong empirical validation. To my reading, he is in danger of paying too little regard to the dynamic nature of interparty conflict. Is interparty conflict not exactly about the relative salience of different dimensions? Both old and new parties seek to exploit new issues, possibly resulting in new dimensions of conflict. Each party tries to exploit to the maximum that dimension in which it finds itself most comfortable, while attempting simultaneously to drive a wedge into the ranks of its adversaries whose main advantage may lie on another dimension. Sani and Sartori offer interesting data on the congruence of attitudes on diverse questions and self-locations on a left-right scale. But even their own graphs show such a relationship to be far from absolute, which must imply that there is ample opportunity for parties to compete selectively with one another (see also Budge & Farlie, 1983). Sani and Sartori single out Belgium as a special case of two-dimensional electoral competition. Is Belgium really unique, or only one rather clear example of multidimensional competition, not unknown elsewhere?

Sartori's propositions need more empirical enquiry, and his theoretical scheme needs further consideration from the perspective of center and center parties as well as in other ways. For Sartori uses not only *spatial* metaphors and insights derived from properties of *perception* (the center becoming salient as an ideological space stretches), but also views about the *mechanical* interaction among parties as the number of relevant actors increases—changing a centripetal into centrifugal competition—and criteria of relative size (e.g., the development of more than two poles in a system, and the occupation or nonoccupation of the center by one or more center parties).

Can a Center Exist in a Nonreduceable Multidimensional Space?

Here we tread on a terrain that is best covered by formal theory. Judging by one study that specifically investigates our problem (Petry, 1978), it seems solvable only if we redefine the notion of the center as some form of equilibrium point in a game-theoretical setting. It appears that such a point can be obtained only if certain specific conditions are set, e.g., considerations of minimum size, familiarity or inertia considerations which focus on established links between actors and which restrict the range of possible coalitions ruling out unfamiliar combinations; and considerations concerning the relative dependence of different preferences of actors on one another, in order to compel certain combinations of stands because they are logically connected, and to rule out others. I fear that at this point I must leave the argument to specialists in formal theories who move in a mathematical world well beyond my horizon.

Is the Spatial Image (Whether Unidimensional or Multidimensional in Nature) Applicable to All European Party Systems?

One could think of two situations in which the spatial representation becomes meaningless:

A situation of complete *Allgemeinkoalitionsfähigkeit*[2] of all parties. In such a situation only office counts, rather than with whom one gets office. Neither past coalition experiences, nor ideological dimensions, nor future policies offer any restraints. This situation is nearest to the reasoning of the coalition theory of the minimum-size variety. That this theory performed most poorly in empirical tests of European cabinet coalitions (Browne, 1970; De Swaan, 1973) suggests that the assumptions are not very realistic. The need to reintroduce ideological dimensions or other restraining conditions or both may therefore yet save the notion of a center in European party systems.

The second situation might be one of complete segmentation, dividing society into closed subcultures between which no logical relations exist. Typically, in such a situation neither normal majority assumptions nor considerations of ideological affinity hold. Instead, government proceeds by *Proporz,* a deliberate depoliticization of issues, a far-reaching autonomy of subcultures.[3]

The Center in Terms of Mechanics

If one moves from spatial reasonings to conceptions drawn from the world of mechanics, one meets with two rather different metaphors. First, one can imagine a set of scales that a particular force tips toward one side or the other. Second, one may conceive of centripetal against centrifugal forces working in a given party system. In either case, one can reason in terms of voters or of parties. In the case of voters, the emphasis is on the outcome of elections. In the case of parties the accent is rather on the formation or breakup of government coalitions.

The Image of the Scales: Floating Voters

A large body of writings—whether on formal models of party systems (e.g., Downs, 1957) or more descriptive surveys of particular countries (one should note the substantial new work of Browne & Dreijmanis, 1982, and Luebbert, 1983)—gives a crucial place to voters in the middle. By moving their weight from one (alternative) coalition of parties to another, voters can determine the composition of governments. One often assumes that voters in the middle also represent a force for moderation. But many analysts question why this must be so; are the floating voters the best informed or the most responsible? Or, on the contrary, are they the least interested and most indifferent? Or should one regard them as those most likely to be subject to cross-pressures, which makes them reject extremist demands on any particular issue? The debate on the floating voters is now at least two decades old (see Daudt, 1961), and it is clear from empirical study that there is not one simple answer. A reification of "the" floating vote does insufficient justice to the fact that it is in practice composed of varied and changing groups of voters. The very image of one "floating" vote pays too little attention to cross-currents, to changes in the makeup of the electorate through the entry of new voters and the deaths of older ones, and to the additional effects of voting or nonvoting. Voters in the assumed "floating vote" represent very different opinions, regard rather different issues as salient, and need not be in the center at all.

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[2] It has been a particular pleasure to watch the development of this term—invented during an academic conference by that famous punster, the late Val Lorwin, to make light of both Belgian politics and German academic language—into a "serious" term of contemporary scholarship.

[3] See the various analyses of Lijphart (1968a, b; 1977, 1979) and Daalder (1974, 1979). It is perhaps intriguing to note that Lijphart rarely uses concepts like center or centrism in his analysis of Dutch politics, whereas my own more descriptive writing abounds with the concept.

Such problems are evaded in another mechanistic metaphor—e.g., the assumption that there is a natural swing of the pendulum. A party or parties in power—so it is argued—cannot escape growing criticism that eventually will lead to a loss of voters shifting to opposition parties. That mechanism holds governments at least indirectly accountable, so that it cannot afford to move far out from the middle ground between it and the opposition. Empirical data (as presented by Rose & Mackie, 1983), do not offer substantial proof for that mechanical conception.

The Impact of the Scales: Key Parties

In a similar analogy, particular parties—of various sizes—are thought to occupy a position in which they can determine the formation (or ending) of a particular government coalition. Their role in this respect has been characterized also with a variety of other mechanistic metaphors such as pivotal parties, key parties, hinge parties, or *partis charnières*. Logically, their special position depends on the circumstance that their cooperation is necessary to form or maintain a government, given a situation that parties on either side of them lack a majority of their own and prove unable to enter into a (larger) coalition between themselves, bypassing parties in between. The argument has been strongly influenced by the traditional finding that before the Fifth Republic France was governed by what Duverger called the *éternel marais* of the Center, and by the postwar position of the Liberal FDP in West Germany. Yet here again questions must be raised. Is it justified to lump together—all and sundry—small parties that can balance two very much larger parties, the size of which makes the inclusion of any one of them in government a foregone conclusion? Should one argue that in an evenly balanced position of rival governing teams any party can develop into a hinge party, irrespective of its own political positioning in relation to the other parties? There are some empirical pointers in that direction (as the wooing of the nationalist parties in Britain in the 1970s. or the rather odd position of the Austrian FOeP in relation to Socialists and Catholics has shown). Yet in that case the mechanical metaphor is pushed so far that what would otherwise remain an out-party is made into a center party by definition. Compared to that type of argument, the earlier assumption about a relatively persistent patterning of parties in relation to one another in known dimensions of politics seems rather more persuasive.

The chief problem with both the voter and the party metaphor based on the image of the scales is the underlying assumption that a party system provides potential rival teams that must be in some form of rough balance with one another. In many actual party systems, that is not a realistic assumption: e.g., in systems with a predominant (let alone a hegemonic) party, or in complex party systems that do not spontaneously divide into two alternative blocs. In the latter situation, the likelihood that movement by a specific group of voters, or by one particular party, can force a drastic readjustment of forces is not very great. How useful is it, anyway, to reason in a metaphor of a set of scales, if a rather large party, rather than a small one, actually straddles the equilibrium point?

Centripetal and Centrifugal Mechanics

We must now return to Sartori's analysis of centripetal competition for voters at the center, which he regards as characteristic for systems with two to four relevant parties—competing along one and the same dimension with a low degree of ideological polarization—and of centrifugal competition, which to him is the mechanical disposition of systems of five or more relevant parties—not being segmented polities—with a high degree of ideological polarization (see in particular Sartori, 1976, pp. 342-351). A major element in his analysis is the view that in the latter case the occupation of the center by one or more center parties encourages centrifugal drives. I offer the following comments.

Sartori has generally taken a rather negative stand toward center parties, although he grudgingly gave these parties a somewhat more positive evaluation in later years. I quote from two different publications:

> In a situation of centrifugal pluralism the center party (or parties) appears to be more than anything else a feedback, or a retroaction, of the centrifugal drives. . . . According to this interpretation, then, the center is more a negative convergence, a sum of exclusions, than a positive agency of instigation. And this is why it is likely to be a passive, rather inert, and—all in all—immobile kind of aggregation. (Sartori, 1966, pp. 164-165)

and

> I still believe in this diagnosis, but the recent Chilean experience—which was characterized by a chronic fickleness of the in-between parties—vindicates a more positive interpretation. I would put it thus: Even though the center parties tend to be immobilistic, they remain an equilibrating force that performs a "mediating role," and mediation, or brokerage, is not the same as immobilism. This having been conceded, I hasten to add that a center positioning seemingly condemns to a policy of mediation, in the sense that a different role backfires on the party's positioning without paying off in performance or accomplishment. A center party that attempts to outdo the parties located on its left or right will contribute, more than to anything else, to a crescendo of escalation and extremization. (Sartori, 1976, p. 135)

Sartori posits that the very occupancy of the center area by one or more center parties puts that area "out of competition," and hence encourages centrifugal rather than centripetal drives. It is difficult to concur with this argument: as long as there are votes in the center, why should parties not in the center refrain from going after these votes in what must by definition be centripetal electoral tactics? Rather than speaking of centripetal competition in systems of moderate pluralism (generally having three to five parties) and of centrifugal competitions in systems of polarized pluralism (having five or more parties effectively competing with one another), is it not more reasonable to argue that in any system of three or more parties (in which at least one party finds itself in an intermediate position between other parties), there will always be *both* centripetal *and* centrifugal drives? In a visual representation of competition among five parties (Sartori, 1976, p. 349), large extreme parties on the Left and Right overlap only with the intermediate parties, not with the large party at the center. But why should large out-parties not attempt to move toward the center at the expense of the intermediate and center parties both? Are not both the PCI and the MSI competing with the DC directly for electoral votes in contemporary Italy? (For criticism of Sartori's position in this respect, see Farneti, 1983, and Sartori's reply to Farneti in Sartori, 1982, pp. 320-322. See also Daalder, 1983.)

Sartori's argument is possibly influenced by the fact that the DC represents a center that is really based on a criterion other than the formal left-right dimension. Yet if so, he should be led to different conclusions by his own view that it is typically the left-right space of competition rather than domains of identification which governs the decisive relations among parties. A comparison of the Italian and the Dutch case would seem to confirm that conclusion. In both systems, the center is occupied and has been so occupied for many years. In the Dutch case, this fact has been a major element in forcing both Socialists and Liberals to tone down whatever anticlerical proclivities they may once have had, and indeed in forcing them to compete both in policies and electorally in above all a centripetal manner (see Daalder, 1966, 1979).[4] Sartori will argue that the Dutch and Italian cases are not really comparable, given the presence of sizeable antisystem oppositions and a much wider ideological space in Italy as compared to the Netherlands. This is of course true, but is not one major reason for the lower polarization in the Netherlands that parties once distant from one another eventually learnt to *compete in a centripetal manner, and hence in the* end contributed toward depolarization?

On a more theoretical plane, Sartori finds one distinguishing feature of "moderate" as contrasted to "polarized" pluralism in the existence of "bilateral oppositions" in the latter against "bipolar mechanics" in the earlier case. In a self-locating left-right scale, the Netherlands comes out as a country that generally shows a number of distinct modes (e.g., Inglehart & Klingemann, 1976; Sidjanski, 1979. One should note, however, that this finding comes out much more clearly with the use of 9- or 10- than with 7-point scales, Niemöller, 1981, pp. 231-234.). Although the weight of the extremes in the Dutch system is very much smaller than that in Italy, it is far from evident that the Netherlands could really be regarded as an example of a "bi-polar coalitional configuration" (Sartori, 1976, p. 179).

One is struck by the fact that Sartori gives relatively little attention to size factors, in that the proportionate size of a center party, of adjacent parties, and of more extreme parties including whenever relevant antisystem parties, may be more important for the actual functioning of party systems than the absolute number of relevant parties. One should attempt to specify the importance of such size factors.

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[4] A point in favor of Sartori's analysis is the sharpening of centrifugal political strategies (in continuous rivalry with centripetal ones) which occurred when the barriers between segmented subcultures began to break down in the Netherlands. For a short discussion see Daalder (1979). Such strategies—practiced particularly by the left—eventually met with failure, including first the unexpected formation of a center-right government in 1977 and then considerable electoral losses by the aggregate older left in favor notably of the D'66 Party. That party loosened itself deliberately from an erstwhile embrace in a left coalition, to choose a decisively centripetal course. This in turn forced the Socialist party—great clamor of some of its activists notwithstanding—to follow suit.

Analyses in Terms of Cleavages

Yet another manner to treat the presence or absence of a center in party systems is through analysis in terms of social and political cleavages. Here we again encounter Duverger, with his reference to the center as "the moderates of opposed tendencies" and also in his derivation of the center from a "superimposition of dualisms." The following comments seem in order. (See also Lijphart, 1981.)

The Dangers of the Cleavages Terminology

The term *cleavage* has the unavoidable connotation that there is a sharp divide that separates social or political groups. It follows that the middle ground is a dangerous place to be, or even one that is logically nonexistent as indeed Duverger would have us believe. (For an interesting analysis of the origins of the dualist interpretation of French politics, see Graham, 1964.) However, if one inspects the nature of actual political and social cleavages, it becomes apparent that the idea of a clear divide is much more applicable to certain categories of cleavages than to others. There is indeed little between blacks and whites, Catholics and Calvinists, native speakers of one tongue or another, or members of different tribes. But such a statement is not true for many other social bases on which parties have been formed, e.g., the criteria that to Hume were the three alternative bases of party: interest, affection, and principle; social class (except in a vulgarized Marxist sense); regionalism, or even some of the new social cleavages arising from a reorientation of values in a postindustrial society toward the environment, women's rights, issues of organizational scale, international peace, or special concerns with the Third World. A realistic analysis in terms of cleavages should lead one to conclude therefore that centrist and intermediate positions are a much more realistic proposition for some cleavages than for others, without succumbing immediately to what one might call the tyranny of polar types.

The Importance of Numbers

Even if political life were organized solely around exclusive cleavages, it makes considerable difference in practice whether it leads to the formation of two groups or more. Compromise and adjustment would seem much more easy to reach when more than two ascriptive groups are involved, and especially when no single group has a definite majority position. This finding is indeed one common in both anthropological literature and in the writing on modern segmented polities (e.g., Lijphart, 1977). When one group has an unassailable majority, there is much less scope for brokerage than when numerical relations make some form of bargaining inevitable even to reach a majority point.

The Degree of Political Affect

One further disadvantage of a reasoning in terms of cleavages is that one concludes too rapidly that particular cleavages are unavoidably loaded with political content. It may be true that survey data demonstrate highly skewed patterns which suggest that different groups are opposed on matters like religion, class, or language. Yet the political relevance of such a finding depends on the actual degree to which such cleavage divisions are politicized, a point underlined also by Sani and Sartori (1983) when they argue that conflicting identifications need not result in effective political combat.

The Superimposition Hypothesis Once More

Duverger's derivation of a center through a superimposition of dualistic cleavages logically presupposes that particular cleavages have different salience for different groups of political actors. By *assuming* one cleavage to be the dominant one from a systemic point of view, groups on another cleavage end up having a heterogeneous, hence by definition an ambiguous, position. This is indeed Duverger's biased view, but tenable only if one accepts the a priori assumption of the existence of one overriding dimension. It is a telling illustration of the powerful normative sentiments that accompany discussions of a center. What is detested by Duverger as preventing "real" choices is hailed elsewhere under the perspective of "cross-cutting cleavages," regarded as the force *par excellence* which ensures moderation in a homogeneous political culture!

The Center as a Historical Residue

In yet another generalization, Duverger explains parties in a center position as the result of new parties springing up to the left of existing ones, which force the previously existing parties from their original left position to the center or even the right of the system. One is probably not far amiss in thinking that Duverger sees the new parties as representing the future, relegating older parties to eventual oblivion as no longer representative of living forces. The argument underplays the possibility of new parties coming in, not from the Left but from the Right, or even into the middle of the existing political spectrum (e.g., in Duverger's France the Gaullists and reforming

Centrists, D'66 in the Netherlands, or the not easily placeable Progress Party in Denmark).

Even if one were to accept Duverger's law of a gradual displacement of older parties by newer parties on their left (i.e., in the light of the historical rise of new working-class parties), one should recognize that older cleavage lines are not necessarily doomed to extinction. One famous example is the course of events in Norway, where the original Left of peripheral protest parties was indeed shifted away from its original position to such an extent that one wing (the Agrarians) was eventually to re-name itself Center Party. Yet as Rokkan and Valen had correctly forecast (1962, 1964; see also Rokkan, 1970; Valen, 1976), older cleavages could indeed become salient again once their values were threatened. In the 1972 battle over the EEC Referendum, many of the presumed Centrists joined modern New Left groups in polar opposition to a modern pro-European front of conservatives and estalishment socialists alike. Clearly, one must reason not in terms of a static superposition of dualisms, but in terms of a dynamic interplay of cleavages, historically *and* in the contemporary world. History does not freeze parties in immovable positions, nor does it force them into an irreversible momentum in one direction only.

Labels and Positions: The Concrete Experience of European Party Systems

So far, our search for the center has mainly followed conceptual lines. We have explored spatial analogies, ranging from a concentration on the median voter or median party, moving on to the assumption of the existence of a left-right dimension (which easily suggests the notion of a center location as well), and ending with the intractable problem of a multidimensional political universe. We have turned toward mechanistic metaphors in terms of a balancing of the scales and of centripetal versus centrifugal forces. Finally, we have concentrated on analyses of cleavages which can be more or less dualistic, and hence allow or not allow for intermediate or brokerage positions. Our search has been far from conclusive. Our most difficult problem has been that of the inevitable multiplicity of issues, dimensions, and cleavages in political life which stubbornly resist attempts to reduce them to one dominant underlying dimension. (If that analysis is true, then one should query the facile use of the terms *left* and *right* as much as that of the term *center,* as all such terms become inherently ambiguous.) A further problem is the mixing of metaphors. Finally, there are clear signs of bias. Whereas some like a center, others do not. And with Sartori, one may even frown on center parties because one does like competition for the center.

Can one do better by inspecting the actual record of European party systems? One could do so by investigating to what degree the use of the term *center* by particular parties in particular party systems throws light on our problem, and by exploring the extent to which European party systems are in practice characterized by having parties at the center or not.

Checking the Labels

There is little doubt that the label *center* has gained in popularity among European parties in recent years. In Nordic countries a number of parties formally took on the label *center* as former agrarian parties renamed themselves Center Party in Sweden (1957), Norway (1959), and Finland (1965). In Denmark a splinter party from the Socialist Party called itself the Center Democrats (1973). In the re-emerging democracies in Southern Europe at least one party adopted the label *center,* although this label covered rather different parties in different countries, e.g., Suarez's Union of Democratic Center in Spain, the Democratic Social Center (now Christian Democratic Party) in Portugal, and the Center Union of Makros in Greece. (Duverger would have noted with satisfaction that neither parties nor labels proved durable, however. See Di Palma, 1978.)

Appeals to a center designation are also made in other countries, even though parties did not take on the actual label. Thus, in Italy the DC, the PRI, and the PSDI have claimed the presumed center space in the political system, and even the Liberal PLI has sought to out-center the DC from the right in recent times. In French politics a center tendency has been a consistent phenomenon, and the label has constantly reappeared for one or other group within it (see Irving, 1976, and the excellent essay of Bartolini, 1981). One finds a general usage of the term in Dutch political dialogue. There was widespread discussion in Britain of the need for a Center Party before the new Social Democratic Party ws formed in 1981 (e.g., *The Times,* January 17, 1980, following Roy Jenkins's Richard Dimbleby Lecture of November 22, 1979).

The concept *left* has been so strong, however, as practically to preclude the use of the term *center* by parties that find themselves hemmed in by a large party on their left (whether Communists or Socialists or both), and large parties on their right. Non-socialist reform movements have generally preferred terms like *radicals, democrats,* or *progressives* to that of *center.* In some cases parties that have willy nilly found themselves in what might easily be regarded as objectively a center position have deliberately disclaimed the label that they found uncongenial for ideological reasons (e.g., the Italian PSI). Even academic writers unblushingly dichotomize European parties into left and center-right parties, clearly regarding the label *center* as being more right than left (Budge & Herman, 1978; Morgan & Silvestri, 1982).

Checking the Positions

One can also explore when and where parties have in fact acted as center parties in European party systems, irrespective of their labels. The following attempt at a simple classification is based mainly on the mechanistic assumption of the presence or absence of one or more parties which can effectively tip the balance to one government combination rather than another.

In a pure two-party system (such as the stereotyped British one), the idea of a center party would not logically fit. At most, with Downs and Sartori one should argue in such a situation in terms of the center as a point of gravitation to which the two competing parties would be irresistibly drawn. The traditional interpretation of British politics in terms of a Front Benchers' Constitution (Wheare, 1954) where the "Butskells" rule, whatever the more militants in either party say, illustrates—and probably inspired—this way of reasoning. But at least two developments have since then changed this classical conception: the loss by Labour of a majority position in the Commons in the 1970s, and growing concern about the Downsian movement of the two main system parties away from the center. The first event gave life to the idea that other parties might occupy a balancing position (e.g., the Liberals, but also the various nationalist parties and, since 1981, the new SDP c.q. the Alliance). Resistance against a growing adversary politics, on the other hand, has done much to upgrade the view that what Britain needs are not majority parties, but center parties (e.g., Bogdanor, 1983; Finer, 1975, 1980).

A second case has been dubbed a *two-and-a-half party* system, i.e., a system with two large rival parties, each of which needs the support of a third party to obtain a majority in parliament. The archetype is of course the German Federal Republic since the early 1960s, where the FDP has used its leverage to obtain representation in national and *Länder*-governments well beyond its proportionate share. As compared to the German FDP, the Austrian Freedom Party seemed to be too far removed from what would be regarded as a legitimate center position to play a similar role. Yet with the fall of the Kreisky Cabinet in 1983, the strength of mechanical factors was such as indeed to propel that party into government with the Socialists. Could Irish Labor eventually develop similarly into a third force between Fianna Fail and Fine Gael?

In a number of European systems a situation has occurred where party conflict consisted of *one large party facing a number of smaller parties.* The clearest examples of such a situation have been Ireland and Sweden, where Fianna Fail and the Socialists face a number of much smaller rivals. Somewhat similar situations occurred for a time in Norway and in the German Federal Republic (when the CDU/CSU crossed or came near to the majority point). Often the out-parties had little choice but to remain opposition parties in such a situation, ruling out the possibility of regarding them as center parties. Yet such a situation presents a standing temptation to one or other of the smaller opposition parties to move over, as indeed the Agrarians did repeatedly in Scandinavia even before they called themselves center parties.

In a fourth situation conflict has been mainly between two rival blocs, as in the traditional two-bloc parliamentarism of Denmark, or in the more recent period of the French Fifth Republic. In such a situation, the movement of a single party can be of decisive importance. Witness, for example, the change in the balance of Danish politics when the Danish radicals went over decisively to a bourgeois bloc, or the flirtation of some socialists and majority centrists with the idea of a new center coalition, before the victory of Mitterrand in 1981 put an end to such speculations for the time being. What clearly distinguishes this type of political situation from the third situation mentioned above is the absence of a realistic expectation of an independent majority for any one party. Hence, there is room for tactical maneuvering for what is intuitively described as the Center-Left and the Center-Right to move either toward each other, or each to a possible ally in a centrifugal direction, which underlines the importance of quantitative proportions in addition to locational ones. It also suggests that terms such as *centripetal* and *centrifugal* may have a different explanation, depending on electoral or coalitional politics.

In other countries—or at different times in countries mentioned earlier—the typical situation has been one of continuous (or at least very frequent) rule by *one or more parties that have changed allies from time to time* (for data on the period from 1918 to 1969, see Daalder, 1971, p. 290; for a similar table on the period from 1946 to 1980, see Von Beyme, 1982, p. 393). The typical cases are those of the Netherlands, Italy, and to a degree also Finland. The hold on the system by the Catholics (and to a large extent their Protestant allies, now all amalgamated in a new

Christian Democratic Appeal) in Holland, of the DC in Italy, and of the Finnish Agrarians who were later to call themselves explicitly the Center Party, has been such as to make them of pivotal importance to the system. Yet within this category certain differences exist, depending on the extent to which these parties can actually enter alternating coalitions (the case of semiturnover, typically found in the Netherlands), or are forced to look mainly in one direction (Italy, ever since parties on the right of the DC were regarded as clearly illegitimate, thus leaving only the choice between minority government and an *apertura a sinistra*).

In this category one should also place most of the French Fourth Republic and Belgium and Luxemburg, even though the role of the Christian Democrats has been somewhat less dominant in these countries than that of their Dutch counterparts.

Some countries resort frequently to the practice of *minority governments* (cf. Daalder, 1971; Herman & Pope, 1973). Parties frequently called to exercise the role of government in such countries could conceivably be regarded as center parties to the extent that their presence in government, paradoxically, is resented least by all other relevant parties. However, for an understanding of the nature of actual interparty relations, the operation of parliamentary coalitions may be just as telling as the presence of a particular party in (minority) government.

Finally, there is a residual class which for want of a better term one might call that of *changing coalitions*. Relations of parties are such that neither clear alternation nor semiturnover is present sufficiently to constitute a regular pattern. The chief candidates for this category would seem to be Iceland, but also Finnish and maybe Belgian politics offer some illustrations. Typically, parties may ally themselves along frequently changing dimensions and opt for grand coalitions as much as for caretaker governments of experts or minority parties.

The Relevance of the Notion of Center Parties for Different European Party Systems

If the short inventory given above is correct, European party systems seem to fall in at least three different groups.

In some systems the notion of a center party hardly applies. This is logically the case under any of three conditions: countries with stable single-party majority governments (e.g., Britain, France under De Gaulle and Pompidou, Austria under Kreisky, Sweden until at least 1976), countries with clear alternating governments even when often not of the single-party majority variety (Denmark until 1973, Norway, Ireland), and in the rather different case of *Proporz* systems, in which all major parties enter government by definition (e.g., Switzerland).

In a second group of countries, the idea of center party or center parties is a key variable. This was the case in the French Fourth Republic and has been true of Italy, Finland, and also the Netherlands ever since the religious system parties forced Liberals and Socialists into the position of alternate suitors. It is also true for the German Federal Republic as long as the FDP can effectively play off Christian Democrats and Socialists against one another.

Finally, there is a third group of countries where the relevance of the notion of center parties is contingent on both size and positioning of major parties in relation to one another, which is dependent on the outcome of elections and the issues to be handled. In this intermediate category one might place Belgium, Luxemburg, Denmark since 1973, and possibly Iceland.

As this grouping suggests, the major determining variables are the relative size of parties (above all, majority or minority) and how stable the parties' positions are in relation to one another. Countries differ markedly from one another in both respects, and a particular country may also change from one category to another over time.[5]

Power and Prejudice

The preceding pages leave two possible approaches still untreated.

First, one might pay special attention to parties continuously in power. Continuous exercise of power is clearly not the same as being a dominant party, since the first notion is compatible with the continuous presence of a *small* party in successive government coalition. Such a categorization points to a group of parties which provide important questions for detailed study, e.g., of the forces that make such a party indispensable for governments, and of the consequences that result from long government tenure for the electoral position of the party, for its role in relation to other political actors (notably the bureaucracy), as well as the consequences that result from its relationship to parties not in the government. But equating center parties with perennial governing parties otherwise confuses rather than clarifies understanding.

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[5] If the role of a center party depends above all on the relative size of parties in relation to one another, this would logically imply that specific parliamentary situations rather than countries are the more promising units of analysis.

Second, we clearly need an explicit study of normative assessments of center parties. We have frequently drawn attention to the many unspecified value judgments associated with the treatment of notions like center and center parties. A careful review of such value judgments might help us to obtain clearer insights into alternative theories of good government, which after all lay a heavy hand on our discussions and comparative research on the empirical functioning of party systems.

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The Constituency Service Basis of the Personal Vote for U.S. Representatives and British Members of Parliament

BRUCE E. CAIN
California Institute of Technology

JOHN A. FEREJOHN
Stanford University

MORRIS P. FIORINA
Harvard University

Under the guise of the "incumbency advantage," American research of the past decade has devoted heavy emphasis to what may be termed the "personal vote" in congressional elections. Is this phenomenon purely American, or is it susceptible to comparative treatment? This article contrasts the personal vote in the 1980 U.S. House elections with that in the 1979 British general election. The analysis utilizes data from surveys conducted by the Center for Political Studies and British Gallup in combination with interviews of congressional administrative assistants (AAs) and British MPs and party agents whose constituencies fall in the sampling frames of the mass surveys. The analysis finds an incumbency advantage or personal vote in Britain which is much weaker than that in the United States but of somewhat greater importance than is commonly believed. As in the United States, constituency service appears to be an important component of the personal vote.

During the past decade an especially active research area has developed around the study of the advantages of incumbency in U.S. House elections. Erikson (1972), Tufte (1973), and Mayhew (1974a) first called attention to the temporal increase apparent over the course of the 1960s, and numerous succeeding scholars (Burnham, 1975; Cover, 1977; Ferejohn, 1977; Fiorina, 1977; Hinckley, 1980; Parker, 1980a) have theorized about the bases and the consequences of the trends identified by Erikson, Tufte, and Mayhew.[1] This outpouring of scholarly effort has produced a reasonable understanding of the multifaceted nature of the incumbency advantage in contemporary elections, although the lack of appropriate longitudinal data hinders efforts to determine precisely what and how much has changed over time (Fiorina, 1982).

As with much of the congressional literature, a notable feature of the research on incumbency is its exclusively American perspective. In particular, attempts focus on American political institutions and the American political context. Little effort has been made to compare candidate effects in House elections with those that might be present in the legislative elections of other countries,[2] and virtually no effort has been given to abstracting from the American case in an effort to develop more widely applicable theories of the conditions that enhance or depress candidate effects in legislative elections. This article aims principally at the former, empirical, lacuna. Although the significance of any comparative work depends on some basic theoretical ideas that render comparison meaningful and interesting, a detailed comparative theory of voting in legislative elections lies outside our present scope.

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[1] This extensive literature is cited, discussed, synthesized, and otherwise dealt with in two recent books on congressional elections by Hinckley (1981) and Jacobson (1982).

[2] Pierce and Converse (1981) is a notable exception, albeit one that focuses on candidate visibility rather than the vote. Also to be noted are Tufte's (1973) cross-national comparison of swing ratios, and Stokes's (1967) contrast of "swing" in the United States and Great Britain. Such aggregate comparisons reflect relative differences in district-level forces, if not necessarily individual candidate effects.

The Concept of a Personal Vote

By "personal vote" we mean that portion of a candidate's electoral support which originates in his or her personal qualities, qualifications, activities, and record. In legislative elections especially, political science research emphasizes that part of the vote which is not personal—support based on shared partisan affiliations, fixed voter characteristics such as class, religion, and ethnicity, reactions to national conditions such as the state of the economy, and performance evaluations centered on the head of the governing party. This imbalance in emphasis is reasonable enough; most empirical work suggests that factors such as the preceding account for the lion's share of the variation in election outcomes. Only after American scholars realized that the personal vote had reached significant proportions did they really give it much attention.

Still, even if small, the personal vote has potentially great political significance. In contrast with votes based on party or class identifications, religious affiliations, the national economy, or national executive performance, the individual legislator has by definition some impact on the personal vote, and because it is under his control, he may give it disproportionate attention. This in turn has implications for party cohesion in the legislature, party support for the executive, and ultimately, the ability to enforce national electoral accountability in the system.[3] A personal vote reflects a principal feature of the single-member district plurality electoral system: the distinction between the interests and fortunes of an individual representative and those of any collectivity, especially party, to which he or she may belong. It is logically possible for any given representative to win while all fellow partisans are defeated. This simple fact creates an incentive for each representative to build a personal base of support within the geographic district, support not subject to the vagaries of national swings arising from popular reactions to national events, personalities, and conditions. To be sure, myriad features of a political system may work to circumscribe the operation of the individual representative's incentive—the resources available to him, the nomination system, the electoral system (e.g., independent executive or not), the needs, ideologies, and party loyalties of constituents—to name but a few of the more obvious ones. Thus, the gap between individual and collective interests may be large in some systems (e.g., the American) and virtually nonexistent in others (e.g., the textbook British account). The incentive still exists, however, and fragments of the existing literature gave us reason to believe that it operates even in a system like Great Britain's, although with effects much weaker than those observed in the United States.

The preliminary statistical analysis presented in Table 1 illustrates this discussion. The data are from the 1980 NES/CPS American National Election Study and a British Gallup survey conducted after the May 1979 election.[4] The estimates (probit) show the association of party identification, executive performance ratings, and candidate incumbency status (coded from standard sources and merged with the survey files) with the vote for or against the parliamentary and congressional candidates of the incumbent Labour and Democratic parties. Evidently, the American and British findings differ in several respects. First, partisanship exerts a much larger impact, *ceteris paribus,* in British parliamentary voting than in American House voting. Although this difference is expected, one would imagine that the difference has increased from what it would have been in the late 1940s, for example. A second even more noteworthy difference between the two equations concerns the importance of Callaghan ratings for the fates of Labour candidates, and the virtual irrelevance of Carter ratings for the fates of Democratic candidates.[5] Again, these results are consistent with the tenor of traditional discussions of British voting behavior, and the more recent studies of House elections. From the standpoint of this article, principal interest attaches to a third difference between the two equations: the much greater importance of incumbency status in American House elections than in British parliamentary elections. The differences here are fully as great, *ceteris paribus,* as those between the effects of party identification and executive ratings.

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[3] For an extended development of this argument, see Fiorina (1980).

[4] The 1980 NES/CPS post-election survey included 1408 respondents. The British Gallup survey included 2031 respondents interviewed during the week after the 1979 election in a sampling frame covering England, Scotland, and Wales. In consultation with British Gallup staff we selected a subset of the 1978 CPS/NES items and modified them (when necessary) for administration to a British sample.

[5] Note, however, that the effects of Callaghan ratings are not nearly linear, nor even monotonic. Relative to the omitted reference category, very poor, those who rate Callaghan fair, good, or very good are significantly more likely to vote Labour. Strangely, the small group of voters who offer no opinion of Callaghan are as positively disposed toward Labour as those who rate Callaghan very favorably. Consistent with the argument of Converse (1966), these are individuals with lower educational levels and little knowledge of public affairs.

Table 1. Summary Vote Equations, Great Britain and United States

		Great Britain (n = 1527)	United States (n = 711)	
Party identification	Strong con	–1.86**	–.89**	Strong Rep
	Weak con	–1.43**	–.71**	Weak Rep
	Other	– .15**	–.46**	Ind Rep
	Liberal	– .46**	.40*	Ind Dem
	Weak lab	1.34**	.40*	Weak Dem
	Strong lab	2.12**	.83**	Strong Dem
JC job rating	Very good	1.13**	.25	Strongly approve
	Good	1.18**	.06	Approve
	Fair	.80**	.09	Disapprove
	Poor	.09	–	–
	Don't know	1.15**	–.13	Don't know
Incumbency status	Labour	.32*	.78**	Democrat
	Other	– .26*	–.46*	Republican
Constant		–1.59**	–.36	
Correctly predicted		89%	75%	
R^2		.76	.47	

*$p < .05$.
**$p < .01$.

Still, we daresay that most scholars will be less taken by the difference in the importance of incumbency status in the two countries than by the fact that statistically significant effects appear in Great Britain at all. The estimates show that other things being equal, Labour incumbents ran significantly better than Labour candidates who were contesting open seats, and the latter in turn ran significantly better than Labour candidates seeking to unseat incumbents of any other party (open seats—those for which no incumbent ran—are the omitted reference category for the incumbency status variables). Although significant, these effects are not substantively large; Table 2 contains a translation of the probit estimates into probabilities of supporting candidates of the incumbent party as a function of the respondent's party affiliation and executive performance

Table 2. Estimated Probability of In-Party Vote by Party Identity Executive Performance and District Incumbency Status

	Incumbency		
Great Britain	Non-Labour	Open	Labour
Strong conservative–fair	.00	.00	.01
Weak conservative–fair	.01	.01	.03
No party identification–fair	.15	.21	.32
Liberal–fair	.07	.11	.18
Weak Labour–good	.75	.82	.89
Strong Labour–very good	.92	.95	.98

	Incumbency		
United States	Republican	Open	Democratic
Strong Republican–very poor	.04	.11	.32
Weak Republican–very poor	.06	.14	.39
Independent–poor	.24	.40	.70
Weak Democratic–good	.36	.54	.81
Strong Democratic–good	.53	.70	.90

ratings, and the incumbency status of the constituency. Each party identification category is assigned the modal executive rating of that category.

As Table 2 shows, the impact of Conservative party identification was so strong in 1979 that candidate status made little or no difference among Conservative identifiers (the raw data show that *all* of the 81 strong Conservatives in Labour districts voted against the incumbent). This contrasts sharply with the American case, where a one-third minority of strong Republicans reported support of Democratic incumbents. For those not attached to the Conservative party, however, the effects of incumbency status were more pronounced. Voters offering no party identification, for example, were twice as likely to vote for an incumbent Labour candidate as for a Labour candidate running against an incumbent of another party. The figures are similar for Liberals, and even weak Labour identifiers show a nontrivial effect of incumbency status. In the United States, of course, the general effects of incumbency (looking across the rows) are relatively much stronger, perhaps two-thirds or so as great as the effects of party identification (looking down the columns).

Tables 1 and 2 suggest that there is indeed a personal vote for us to compare, contrast, and explain. Given the amount of research devoted to American voting, our emphasis in this article will be on the British. By way of introduction, let us briefly consider several of the components of the personal vote identified in American research and how they may or may not apply to the British case. The first and most obvious explanation of the House incumbency advantage arises from the sheer quantity of electorally productive resources provided to all incumbents, such as staff, office space, long-distance telephone privileges and the frank, and travel (Cover, 1977; Cover & Brumberg, 1982; Mayhew, 1974b; Parker, 1980b), estimates of the value of which range up to a million dollars per term. This factor can hardly operate in Britain because MPs have very little in the way of personal support. The average MP shares a secretary and may work with a party agent in the constituency.[6] Another partial explanation of the House incumbency advantage focuses on the differential campaign funding of incumbents and challengers (Jacobson, 1980). In Britain, however, campaigns are much cheaper, constituency spending is severely limited, candidates do not raise money individually, and spending decisions are more centralized. Thus, the financial muscle of MPs would seem to be a hypothesis that we can safely dismiss (Pinto-Duschinsky, 1980). Third, some authors have suggested that less tangible factors partially explain the House incumbency advantage. One could argue that strong incumbents deter strong challengers, and that incumbent strength is at least to some extent a self-fulfilling prophecy which results when weak challengers are the only ones willing to make the race (Hinckley, 1980; Mann, 1978). Here again, the hypothesis would not appear as plausible for Britain. Unlike American candidates who await the proper time to run, many aspiring MPs look for a suitable location—a winnable, if not safe, district. An important qualification for the nomination in such a district is a reputation as a good candidate, and an effective way to earn such a reputation is to wage a strong campaign in a hopeless district. King (1982) reports that in the 1970s Parliaments, one-half of all MPs had lost at least once before winning their seats, and one-fourth had lost twice or more.[7] Thus, it appears that incumbent MPs are less likely to get an electoral free ride than are incumbent congressmen (MCs), given that ambitious challengers in Britain cannot hope to impress future selection committees by merely going through the motions.

All in all, the foregoing considerations suggest that the personal vote in Britain is very personal indeed. Its existence would seem to reflect the particular characteristics and activities of particular candidates. Such a vote is contingent; it depends on whom the MPs are and what they do. A likely possibility for an important component of this contingent personal vote corresponds to a fourth partial explanation of the House incumbency advantage—constituency service, by which we mean the nonpartisan, nonprogrammatic effort to help individual constituents in their dealings with the larger government, and to defend and advance the particularistic interests of the constituency in the councils of the larger government. In the next section we present new data on constituency service in Britain, some from the mass survey already introduced, and still more from an elite survey coordinated with the mass survey sampling frame. Analogous American data also will be presented. In the fourth section we report statistical analyses that show the importance of constituency service as an explanation of the personal vote. A concluding section relates our findings to discussions of embryonic developments in the British electoral system.

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[6] Butler and Kavanagh (1980, pp. 58,72) report that in 1979 Labour had only 70 full-time paid agents, and in 1978 the Conservatives had 346. In addition, there are part-time volunteer workers, typically party activists.

[7] King finds that in the post-World War II period there has been a steady upward trend in the proportions of MPs with previous election defeats in their background.

Constituency Service in Great Britain and the United States

The textbook portrait of British politics leaves little room for a personal vote. Although MPs appear desirous of retaining their office (the retirement rate in 1979 was approximately 10%), and thus have an incentive to fashion a personal vote, the instruments available to them appear too paltry to permit them to do so. Most MPs are faceless troops in the party ranks who vote in accord with the party whip. They have little or no personal power (e.g., committee based as in the United States) to procure "pork" for their districts or to provide favors to individual constituents. As mentioned, they have very little staff and office resources, and their campaign spending is both limited and largely out of their control. Their parliamentary careers hinge not only on their continued local renomination and reelection, but also, and more important, on the impressions they make on national party leaders. To cap it all off, their constituents can register a preference for the executive only through their vote decisions for Parliament. Consequently, voters naturally pay scant attention to individual MPs and make their choices on the basis of such general factors as party affiliations, class position, and reactions to top party leaders, particularly those who will constitute the government.

The preceding textbook portrait is familiar to American scholars, many of whom use it to highlight a contrasting textbook portrait of the Congress. Like most textbook portraits, however, the British one is painted in bold relief. Strong tendencies become incontrovertible generalizations, and traces of inconsistent evidence seem to disappear. Knowledgeable observers of British politics have long been aware that MPs are not quite so helpless and electorally irrelevant (or at least don't believe they are) as some textbooks suggest. Moreover, the recent literature increasingly focuses on changes in British politics, changes of a kind different from the generalizations of the older textbooks.

A number of British scholars (Butt, 1967; Chester & Bowring, 1962; Crick, 1970) have observed that in the postwar period the average amount of time devoted by an MP to government legislation has decreased and that devoted to representing constituents against the bureaucracy has increased. Barker and Rush (1970, pp. 183-184) report that Labour and younger members are especially prone to emphasize the "welfare-officer" role. The phrase "a good constituency man" has entered the popular literature on voting (Hartley-Brewer, 1976), and the Liberal-pioneered strategy of grass-rooting has received academic notice (Kavanaugh, 1969, pp. 49-50; King, 1974). Relevant data are scarce, however. In an older publication, Dowse (1963) studied an important aspect of constituency relations—surgeries—via a mail survey of 100 MPs. Of 69 responses, only one-fifth of the MPs held no surgery whatsoever, and those with fewer than nine years of service tended to hold them more frequently than more senior members. But Dowse found no relation between electoral margins and frequency of surgeries, and on that basis concluded that constituency work stems from the "genuine desire to win public esteem and to be of service" (p. 336). When queried directly, only one-third of the MPs viewed their activity as electorally profitable.

In contrast to Dowse's findings, our interviews 16 years later indicate that contemporary MPs are considerably more prone to hold surgeries (Table 3).[8] As seen, 37% of Dowse's 1963 respondents held no regular surgery; by 1979 only a corporal's guard did not hold surgery on a regular basis. At the other extreme of the distribution, one-third of Dowse's respondents held surgeries at least every two weeks, whereas well over one-half of our respondents did so. In two decades surgery has apparently become a standard aspect of an MP's life.

Surgeries produce contact with constituents, generally those having some request, grievance, or other claim vis-à-vis the government. Our interviews explored at length the topic of casework; several salient features of the responses are of interest before we proceed to the statistical analyses.[9] As government has grown, one would naturally expect that demands in the form of casework would grow commensurately, but some authors (e.g., Fiorina, 1977) have hypothesized that electoral incentives lead legislators to stimu-

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[8] In each country we attempted to procure an elite interview for each constituency in the sampling frame of the mass survey. In the United States we completed interviews for 102 of the 108 districts in the sample. Our target was the congressional administrative assistant who preliminary research indicated would be the best source of information on office organization and activities. In Great Britain we completed interviews with MPs or party agents or both in 101 of the 133 constituencies included in the sample. When reporting the data, of course, we include only one interview for each of the 101 constituencies in the sampling frame, or in some cases only for the 69 constituencies in which we interviewed the incumbent MP.

[9] The MP responses, along with analogous MC responses, will be discussed at length in a book now in preparation.

Table 3. Comparative Frequency of Surgeries, 1963 v. 1979

	Dowse (1963) %	CFF (1979) %
None	17	4
Ad hoc basis	20	3
Less than monthly	–	4
Monthly	23	25
Every three weeks	6	6
Every two weeks	22	32
Three per month	–	11
Weekly or more often	12	15
N =	65	100

late constituent demands. To be sure there is a broad range of possibilities, from a simple invitation in a newsletter for constituents to write to a given address, to passing out stamped, preaddressed postcards in nursing homes. At any rate, the interviews showed that at least some level of solicitation is the rule (85%) in the United States. The figure is lower (64%) in Britain, although a clear majority indicates some degree of initiative. In contrast, MPs are more likely (50%) to seek publicity for successful casework. Whereas MCs publicize casework in general terms and in the aggregate, they more often (67%) express the sentiment that publicizing the details of cases would be an invasion of their constituents' privacy.

In the United States the most common types of casework are social security and veterans' benefits (almost universally mentioned). In Britain citizen requests and complaints stem most frequently from housing (mentioned by 85% of our interviewees), pensions (72%), taxes (37%), and immigration (22%). Thus, the single most common source of casework in Britain is a program that is legally a responsibility of local government. An overwhelming majority of MPs (83%) report that they do handle such local casework, although a considerable proportion (33%) do so with reluctance. In contrast, a majority of MCs report they do not handle state and local cases, although they would advise constituents on the appropriate officials to contact.

Obviously, MPs are not geared up to handle casework to the same extent as MCs—they have nothing like the extensive, permanent district operations now common in the United States. Moreover, the much smaller size of British constituencies (about 90,000 people on average, as opposed to 525,000 in the United States) would lead us to expect a smaller case load. However, the estimated case loads reported in Table 4 are higher than we had anticipated. Given that some MPs still answer their mail in longhand, their reported workload is quite impressive.

Table 4. Number of Cases Handled per Week by MCs and MPs

	Congressmen %	MPs %
< 20	9	23
21-40	28	23
41-60	18	14
61-80	6	10
81-100	14	8
100+	16	3
MV^a	10	19
N	102	101

^{a}MV = Refused to answer, didn't know.

There is, then, a great deal of constituency-oriented activity apparent in Great Britain. And although we have presented data only on casework, MPs are in their constituencies more often than they hold surgery: the modal MP returns to the constituency at least weekly (many of them, of course, live in London), and more than 80% go home at least twice a month. Partially as a result of this contact, MPs enjoy high visibility in their constituencies. Referring back to the mass surveys, incumbent MPs enjoyed a spontaneous name recall of 66% in 1979, nearly twice the level achieved by MCs.[10] About an eighth of British respondents claimed to have met their MP personally.

Does the constituency attentiveness of an MP have any electoral payoff? Little in the literature bears directly on the question. As mentioned, only 32% of Dowse's 1963 respondents thought that it did. In our survey, however, 83% (57 of 69) answered definitely yes, and another 16% thought that a limited effect was present. Only one MP flatly denied an electoral effect. This response distribution is virtually the same as that for MCs, although very different from that which Dowse reported. Perhaps there has been a major temporal change, or perhaps Dowse's respondents hesitated to commit a seemingly crass admission to paper. Apropos of the latter possibility, consider that Barker and Rush (1970, p. 177) note that MPs queried in 1967 universally believed that their personal reputations and activities had some impact on the vote.

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[10] Approximately 48% of the respondents could recall the name of any challenger for the parliamentary seat, which is much lower than the percentage who could recall the incumbent's, but also more than twice the recall rate for challengers of MCs.

British academics, however, tend to accept the findings reported by Dowse. Like congressional scholars of the 1960s, British scholars appear reluctant to believe that MPs might be motivated to any significant extent by mundane electoral considerations. The edited transcripts of the King and Sloman BBC interviews reflect (and to some extent underlie) the prevailing consensus; they are worth quoting at some length. The first segment comes from a conversation with Shirley Williams, then a member of the Labour shadow cabinet, and Norman Tebbit, a junior Tory MP, although one from an extremely safe seat. The program was titled, "M.P.s and their Surgeries" (King & Sloman, 1973, pp. 13-14):

> *King:* If it takes up so much time, if M.P.s have to write so many letters, if they sometimes find the work depressing, why do almost all members of Parliament hold surgeries? The cynic would say 'in order to win votes, of course.' But the cynic would be wrong. There is no evidence that this sort of careful individual constituency work makes any substantial difference at the time of a general election, and M.P.s know it. I asked Shirley Williams how far she thought her surgery work helped her win the allegiance of the voters.
>
> *Williams:* I don't think that it makes much difference. All you can say is that perhaps you gradually build up a reputation as a conscientious or reasonably hard-working M.P., and that is of some advantage. But with the individual cases I suspect there's almost no influence at all.
>
> *King:* How much advantage—hundreds of votes, thousands?
>
> *Williams:* At most, hundreds.
>
> *King:* Norman Tebbit seemed surprised even to be asked. Had he won any votes that morning?
>
> *Tebbitt:* Do you know I've never thought of it? I can't say that for me the favorite part of my life as a Member of Parliament is being a social worker, . . . But I just regard it as part of my job and, as to whether it wins votes or not, you know, I'm not really particularly interested.

Similarly, in a segment titled "The Case of Flora Genetio" (King & Sloman, 1973, pp. 26-27), we read the following colloquy between King and Roy Hattersley, then a Labour front-bencher:

> *King:* But in the end doesn't all this constituency work, doesn't the writing of all these letters, the holding of surgeries and advisory sessions, boil down to an effort to win votes, to make sure of getting in next time? Roy Hattersley, and I think most M.P.s, would deny this vigorously. How much help, I asked Roy Hattersley, do you think your constituency work is going to help towards your re-election when the time comes?
>
> *Hattersley:* Very little indeed. My re-election when the time comes depends on the standing of the two parties. I hope I shall poll about nineteen or twenty thousand votes. If two or three hundred of those are the result of my constituency work, I shall have done rather well.
>
> *King:* Why, then, does he do the work?
>
> *Hattersley:* I do the constituency work, not for a political bonus, because there isn't a political bonus in it. I do it because it's part of the job.
>
> *King:* Part of an M.P.s job. The non-partisan, non-speech-making, little-publicized part that goes on week in and week out, even when Parliament is in recess.

Evidently, the MPs quoted above do not share the views of most of those MPs we interviewed. Perhaps we were duped, or perhaps our interviewees did not understand the question as we intended it. But then again, perhaps constituency work is a more important concern of backbenchers, who are academically less interesting than frontbenchers. Perhaps, too, prominent politicians are loathe to announce over the BBC that their actions stem from anything but the highest of motives.

At any rate, there are at least three questions that research would do well to keep separate: 1) Do MPs believe that their constituency work has electoral payoffs? Based on our interviews we think the answer is now generally yes. 2) Is the constituency work of MPs motivated primarily by electoral considerations? The academic consensus is probably no, but in any event ascertaining "real" motivations is terribly difficult. 3) Whatever the motivation, does constituency work have an electoral impact? Aside from Dowse, an older study of the electoral strength of "experienced" candidates in 65 marginal seats (Williams, 1966-1967), and a recent study of 18 marginals by Curtice and Steed (1980), there is little research that sheds light on this last question. In the next section we present some findings based on the elite interviews and mass surveys discussed above.

Constituency Service and the Vote: Great Britain and the United States

Both mass surveys pursued at some length the subject of constituency service. Constituents were asked whether they had ever contacted the incumbent and if so, why, whether they had gotten a response, and whether they considered the response satisfactory. In the United States about one in seven respondents (a higher proportion of actual voters, of course) had initiated some communication with their MC; of these, 7% reported that they had requested help, 4% that they sought information, and 4% that they expressed their opinions. In Great Britain 1 in 12 respondents had contacted their MP, with 5% requesting help, 2% information, and as would be expected, fewer than 2% expressing an opinion.[11] Nearly all constituents in both countries reported that they were "very satisfied" with the response, and fewer than 25% reported either no response or dissatisfaction.

In addition to personal experiences, a fifth of the American and a sixth of the British samples claimed they knew of someone else who had contacted their MC or MP (we refer to this as secondhand contact in the discussion that follows). Further, one-fifth of the American sample and one-eighth of the British maintained that they could recall something special the incumbent had done for the district. The probes accompanying this item elicited a very mixed bag of responses by the Americans, with only about half referring specifically to local concerns and programs. In Britain, however, the modal answer, offered by two-fifths of the respondents, is that the MP champions local causes. Smaller, roughly equal proportions mention housing, local industries, aid to individuals in trouble, and the MPs general interest in local affairs.

Each survey included a generalized evaluative item designed to tap the incumbent's relationship to his constituency. First included in the 1978 NES/CPS election study, the item was dubbed "expectation of access." It was intended to capture some aspects of Fenno's (1978) emphasis on the reputation for accessibility and trustworthiness that a representative seeks to develop. We think that the wording of the question makes it a fair general measure of the extent to which a representative is perceived as "a good constituency man." It reads,

> If you had a problem that Representative (your MP) (name) could do something about, do you think he/she would be very helpful, somewhat helpful, or not very helpful to you?

In both countries constituents expressed fairly positive expectations.[12] Some indication that these expectations have real content and are neither purely random nor pure rationalization appears in Table 5. The figures in the table are probit estimates for statistical models in which expectations of access are the left side variable. The models presume that incumbents enhance their images by achieving visibility and by actually compiling a good record, or at least one that is perceived as good. In addition constituents may have more positive expectations about an incumbent who shares their party affiliation. Conversely, a visible challenger might dim the luster of the incumbent, given that the former may attack the incumbent's record or person as part of his or her campaign. All of these suggestions are no more than common sense, and all are reflected in the data.

The British and American equations are quite similar. MPs may get more political mileage from personal contacts than MC's,[13] and MCs perhaps more out of secondhand contacts (i.e., contacts with friends, relatives, and co-workers about whom the respondent has heard). After taking reported contacts into account, spontaneous name recall appears to have little effect in either country.[14] Party affiliations are more important, *ceteris paribus,* in Britain, with minor party identifiers significantly less likely to evidence positive expectations than even those who identify with

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[11] Munroe (1977) similarly finds that only a small proportion of constituent approaches to MPs involve general issues as opposed to personal concerns. In addition, Barker and Rush (1970, p. 175) comment that "we noticed that every one of a very varied group of constituencies produced more letters raising personal cases and problems than offered opinions on local or national issues."

[12] Across the response categories—very helpful, somewhat helpful, not very helpful, don't know, and depends—the American distribution was 27%, 34%, 10%, 25%, 4%, whereas the British distribution was 28%, 28%, 11%, 24%, 10%. The analyses in Table 5 do not include the "don't know" and "depends" responses. The vote analyses in Tables 6 and 7, however, represent these categories, along with the three ordinal categories, as dummy variables.

[13] The contact variables are created from the following survey item: "There are many ways in which MPs (MCs) can have contact with the people from their constituency. On this page are some of these ways (respondent receives card). Think of (name) who has been the MP (MC) from this constituency. Have you come into contact or learned anything about him/her through any of these ways?" Based on Parker's (1981) analysis the responses were used to create two dummy variables: personal contact (met the incumbent, heard him/her at a meeting, talked to staff, agent, secretary or other employee), and media contact (mail, newspaper/magazine, radio, TV).

[14] The U.S. equations in Tables 5 and 6 were also estimated using name recognition in place of spontaneous name recall. Generally the former has a larger and more highly significant coefficient, but other coefficients in the equations are no more than .02 different, and the overall fit of the equations is no better. Thus, in order to maximize comparability we report only the American equations using name recall. We also included campaign spending in the American equations, but failed to find significant effects. Spending presumably purchases contacts and visibility, but direct measures of the latter already appear in the equations.

Table 5. Expectation of Access Equations: United States and Great Britain

	United States ($N = 811$)	Great Britain ($N = 821$)
Contact		
Personal	.36**	.56**
Media	.39**	.23**
Secondhand	.24*	− .02
Casework		
Very satisfied	1.07**	.92**
Somewhat satisfied	.17**	− .60*
Not satisfied	−1.22**	−1.39**
Secondhand casework		
Satisfied	.66**	.57**
Somewhat satisfied	.02	
Not satisfied	− .67*	
District service	.38**	.55**
Party identification		
Independent	.02	—
Minor	—	− .44*
None	—	.24*
Same	.19*	.41**
Recall incumbent	.16	.05
Recall challenger	− .05	− .02
Year elected	− .01*	.01
Constant	1.25**	.14
$\hat{R}^2$	.36	.29

*$p < .05$.
**$p < .01$.

national parties different from the incumbent's (the latter constitute the omitted reference category in the set of dummy variables).[15] Identifiers with the incumbent's party are the most sanguine about the likelihood that he or she would help in a pinch. Finally, in the United States senior incumbents are expected to be more helpful than junior ones; no comparable relationship is apparent in Britain.

However, the largest coefficients in Table 5 reflect the effects of the incumbent's previous efforts. Satisfied constituents are highly positive about his or her future potential, and dissatisfied constituents (rare) highly negative (the omitted reference category for these dummy variables comprises those who report no casework experience).[16] Those who recall something already done for the constituency are likewise very positive. Not surprisingly, these figures show clearly that incumbent representatives can behave in a manner calculated to enhance their constituents' images of them, and that conclusion holds for MPs as much as for MCs. As yet, however, we do not know the degree to which positive images translate into supportive votes.

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[15] In Tables 5 and 6 party affiliations are coded as follows. In the American sample all respondents fall into mutually exclusive classes: same party as incumbent (51%), independent (14%), opposite party from incumbent (35%). In the British sample 38% share the party affiliation of the incumbent, and 17% admit to no party identification. The opposite party category includes adherents of any party whose MP is not of that party—45%. In order to pick up any additional differences between national and minor party identifiers, an additional dummy variable, *minor party identifier*, is included. This variable takes on a value of one for those 2.5% of the respondents who report an identification with other than the Conservative, Labour, or Liberal parties. To avoid statistical degeneracy in the analyses, one category, opposite party identifier, is omitted from each equation reported in Tables 5 and 6.

[16] We did not get a measure of satisfaction with secondhand casework experience in Britain. Thus, the dummy variable takes on a value of one for all those who report knowledge of friend, relative, or co-worker experience. The large and highly significant coefficient suggests that the effects of satisfactory secondhand experience are very strong, given that the estimate in the table is watered down by inclusion of a presumed minority who recall unsatisfactory experiences.

Table 6 represents a first attempt at answering the preceding questions. The equations reported in this table treat the vote decision as dependent on the visibility of the incumbent and challenger, the reputation of the incumbent for being "a good constituency man," the party affiliation of the incumbent vis-a-vis the constituent, and evaluations of the executive.[17]

Taking the more familiar American results first, the estimates are consistent with recent accounts of House elections in the academic literature, the popular press, and the laments of political leaders. By achieving visibility and developing a reputation for constituency service, the MC can exert a major impact on his or her electoral fate. All else being equal, a very favorable image as a good constituency representative is more important to the candidate in determining the vote than having the same party affiliation as the voter. As in Table 1, the effects of Carter ratings on the House vote in 1980 were nil, even though the present analysis is restricted to incumbents who have a record vis-a-vis Carter.

And what about Britain? The estimates demonstrate much that anyone would have expected, and perhaps a bit that some would not. As in Table 1, the effects of party identification are nothing short of massive (recall that "opposite party identifier" is the omitted reference cate-

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[17] In the analyses reported in Table 6, the executive performance ratings are collapsed into three categories: approve (very good, good), fair/don't know, and disapprove (very poor, poor). Because the effect of executive approval on the vote will depend on the party of incumbent—relating positively to votes for Labour MPs and negatively to votes for Conservatives—we have formed interaction terms between performance ratings and the party of the incumbent MP, which yields six dummy variables, one of which—conservative incumbent, disapprove Callaghan—is omitted from the analysis. Analogous procedures were followed in the American case, although the results are completely insensitive to how or even whether Carter ratings enter the equation.

Table 6. Incumbent Vote Equations, United States and Great Britain

	United States (*n* = 644)	Great Britain (*n* = 1,111)
Incumbent name recall	.44**	.46**
Challenger name recall	– .77**	– .35**
Challenger contact	– .57**	–
Expectation of helpfulness		
Very	1.76**	.35*
Somewhat	.82**	.07
Don't know	.43*	– .22
Depends	–	– .02
Party identification		
Independent	.57**	–
None	–	.99**
Other	–	.99**
Same	1.19**	2.43**
Executive job rating		
Out-party incumbent		
Approve	.23	.01
Fair/DK	– .47	.34*
Disapprove	– .29	.50*
In-party incumbent		
Fair/DK	– .76	– .20
Disapprove	– .13	– .60**
Constant	– .71**	−1.65**
$\hat{R}^2$	.51	.64
Correctly predicted	80%	86%

Table 7. Estimated Probabilities of Incumbent Vote by Expectation of Helpfulness

Incumbent's party	Voter's identification	Not helpful	Somewhat helpful	Very helpful
Great Britain				
Lab	Lab	.89	.91	.94
	None/Other	.34	.37	.48
	Cons	.06	.07	.11
Cons	Lab	.12	.13	.20
	None/Other	.56	.58	.69
	Cons	.95	.96	.98
United States				
Dem	Dem	.58	.90	.99
	Ind	.39	.71	.93
	Rep	.20	.49	.82
Rep	Dem	.32	.63	.90
	Ind	.33	.65	.91
	Rep	.58	.84	.97

gory). How difficult it is for other influences to have an impact in the face of such strong partisan effects is shown in Table 7 (below). The other major influence on British voting decisions offers a clear contrast to the American results. Ratings of Callaghan's performance have significant effects on the vote for MPs. The omitted reference category is "in-party incumbent, approves Callaghan performance" (the expected effects of Callaghan ratings on the vote are obviously conditional on the party of the incumbent MP). Thus, the estimates show that constituents who disapprove of Callaghan's performance are significantly more likely to vote for a Conservative MP than those who approve; constituents of a Labour MP who disapprove of Callaghan are significantly less likely to vote for the MP than are those who approve of Callaghan.

Of most interest, however, are the variables that capture aspects of the personal vote in Britain. As in the U.S. equations, incumbent visibility has a positive and highly significant impact on electoral support (and challenger visibility has a comparable negative impact). Well-known incumbents do better than unknowns, other things being equal. Of even greater interest is the estimate attached to a reputation for constituency service. Those constituents who hold highly positive expectations of their MP are significantly more likely to vote for him or her than those not holding such expectations. There is no denying, however, that the effects of constituency service are but a shadow of what they are in the United States. Table 7 gives some idea of the comparative magnitude of the effects; the variable of interest is the voter's expectation of helpfulness. We examine six configurations obtained by crossing three party-identification categories with two incumbency categories. The figures in the table are calculated from Table 6 under the assumption that the voter has the modal value for variables other than expectations of helpfulness. In the British calculations this means that the voter is assumed to recall the incumbent, not recall the challenger, and rate Callaghan good if a Labour identifier, fair if a nonidentifier, and the average of poor and fair if a Conservative identifier. Given these conditions the voters' estimated probabilities of voting for the incumbent MP are given in the top part of the table.

The party identification and Callaghan rating effects are so strong that the vote is almost a foregone conclusion in many cases, but at the margins the effect of being "a good constituency man" emerges. The smallest effect is a .03 increase in the probability that a Conservative identifier would support a Conservative incumbent, whereas the largest is a .14 increase in the probability that a nonidentifier would support a Labour incumbent.[18] These numbers contrast with the American figures in the bottom half of the table.[19] There we see that an incumbent's perceived reputation can have enormous effects. Consider, for example, the range of probability estimates for identifiers of one party who have a MC of the other: these probabilities triple as a function of perceived reputation. All in all, an MC's reputation for helpfulness appears to have a potential impact as great as that of party identification.

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[18] That is, .03 is the difference in estimated probability of a Conservative identifier supporting a Conservative incumbent whom he believes would not be helpful if a problem arose and that of supporting a Conservative MP who would be very helpful. The other differences mentioned in the text are analogous.

[19] The American figures are calculated under the assumption that the voter does not spontaneously recall

How large is the personal vote in Britain? Is it the negligible few hundred that some observers dismiss? Using Tables 6 and 7 to arrive at a precise estimate is not easy, inasmuch as the estimates vary considerably with voter characteristics and attitudes, but for illustrative purposes imagine some hypothetical races. Looking across our sample we find a constituency quite negative about the expected helpfulness of their MP: 8 electors distributed 0, 1, 7 across the categories "very helpful," "somewhat helpful," and "not very helpful." Another constituency is distributed in exactly the reverse fashion: 7, 1, 0 (a few constituencies have everyone in the first category, incidentally). Applying the probability estimates given in Table 7, and weighting by the actual distributions of party identification in Labour and Conservative incumbent constituencies, respectively, we arrive at estimated differences in expected vote of a little less than 6.5% in the case of Conservative MPs and a little more than 6.5% in the case of Labour MPs. We emphasize that these are *not* estimates of the actual personal vote in 1979, even in an "average" constituency, but rather illustrations of the potential difference between the vote-attracting abilities of MPs with reputations as excellent constituency men and those with reputations as very poor ones. Still, because it is at least partially an MP's decision to become known as an excellent constituency man or a very poor one, the figures represent maximum bounds on the personal vote in contemporary Britain.

We recognize, of course, that some readers may be skeptical of the kind of exercise just reported, inasmuch as it relies on a survey item that measures voter evaluations, and such items always entail a risk of contamination from other evaluative factors. Even given the results in Table 5, should we not worry that responses to the expectation-of-access item are in some part rationalizations, that people who plan to vote for a given incumbent naturally say that he would be very helpful? As a precaution against this possibility we report a second analysis based on the elite surveys discussed in the preceding section. Many of our interviewees voiced the opinion that diligent constituency work could dampen swings against their party or augment swings to their party. Given the data they reported, it is a fairly straightforward matter to examine the accuracy of their beliefs. We formulated a simple additive index based on the MP's description of his constituency work. The index gives a value of one for each of the following: does the MP encourage casework, does the MP publicize successful casework, does the MP handle local cases, and does the MP hold surgery more than twice monthly? The 101 districts for which we have interviews range from zero to four on this index.[20] Do these accounts of constituency work bear any relation to objective swings in the vote? Table 8 shows that they most certainly do.

A noteworthy feature of the 1979 general election was that the traditional uniform swing was much less uniform than usual: North Britain swung to the Conservatives by 4.2%, whereas South Britain swung by 7.7% (Curtice & Steed, 1980, p. 395). Because of regional variations, recent analyses of British electoral behavior have used regional swing figures rather than a single national average. We follow this practice in the analyses reported in Table 8 by regressing the swing in our sample districts on the swing in their larger region, several demographic variables previously identified as important (Crewe, 1979), and their score on the constituency work index.[21] The results are quite indicative. The statistically significant estimates imply that variations in constituency work (0 to 4) account for swings of something between 1.5 and 2% for Conservatives and of something between 3 and 3.5% for Labour. Thus, depending on the party, variations in constituency attentiveness have an electoral effect potentially as large as one quarter to one-half of the observed regional swings. The figure for Labour is almost twice that for Conservatives, consistent with the estimates of Williams (1966-1967) for an earlier period.[22] The estimates are

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the incumbent or challenger, approves of Carter's performance if a Democrat, and disapproves if an Independent or a Republican. Again, these assumptions reflect modal responses in the sample.

[20] The actual distribution of the 101 districts across the 0-4 scale was 16, 21, 28, 31, 5. The analysis in Table 8 utilizes 85 of the 101 cases, excluding retirees, seats won in by-elections during 1974-1979, and seats held by Liberals and Nationalists.

[21] The details of this analysis are discussed at length in Cain (in press).

[22] Williams's (1966-1967) analysis did not utilize a measure of constituency effort. Rather, he sought more broadly to estimate the personal votes of "familiar" Labor versus "familiar" Conservative MPs. A

Table 8. Effect of Constituency Work on Swing[a]

	Conservative Seats		Labour Seats	
	(1)	(2)[b]	(3)	(4)[b]
Regional swing	.59**	.56**	.83**	.79**
Constituency work	.42*	.44*	– .74†	– .88*
Immigrant (%)	–4.24**	–4.03**	–1.17	–
Metropolitan cities	.19	–	2.23*	1.92*
Nonmetropolitan cities	– .89	–	.17	–
Constant	1.15	1.22	2.45	3.06
n	55	55	33	33
$\hat{R}^2$	.41	.39	.55	.52

*$p < .05$.
**$p < .01$.
†$p < .10$.

[a] Swing is defined as the average of the gain in Conservative share of the vote and loss in Labour share. The figures are drawn from the *Times Guide to the House of Commons.*

[b] Equations (2) and (4) omit nonsignificant demographic variables included in equations (1) and (3).

realistic bounds on the actual size of the personal vote, moreover, since it is well within the capability of the average MP to determine where he or she scores on the index of constituency work. We should also note that these estimates are generally in the ballpark, although somewhat larger than those calculated by Curtice and Steed (1980, p. 409) from analysis of 18 "switched" districts.[23]

Discussion

The estimates reported in the preceding section do not show that constituency work is a major influence on the vote in Britain. Rather, our analyses confirm the standard findings that party allegiances and evaluations of party leaders account for the lion's share of the variance in electoral decisions, although it seems clear that party loyalty accounts for less than it once did (Crewe, 1974). Is it the case, then, that constituency service in Britain is of only mild academic interest, not deserving of anything like the attention it has received in the American literature? In our opinion, no.

In the first place, what is of importance to tenured professors seeking to explain variance, and what is of importance to elected officials seeking to win reelection may not correspond very closely. Individual MPs can do little or nothing to alter their constituents' evaluations of party leaders. But individuals can affect their images in the constituency, and the little they can affect may be of greater importance to them than the great deal that they cannot. Moreover, within the ranks of elected officials, there are further distinctions. The minister sitting in a safe seat may share the professor's disdain for a piddling personal vote, but the ambitious politician in a marginal seat may view those one thousand to two thousand votes as the difference between a successful political career and oblivion.[24]

Second, constituency service in Britain might be of more importance than its present impact on the popular vote would indicate, owing to indications that service activity is a growth industry. Our elite interviews contain numerous suggestions that "this sort of thing" has become a larger part of the MP's job in recent years—sometimes to the dismay of older MPs. Constituency parties increasingly require their MPs to establish a local residence. And, as discussed earlier, there has been an increase in the frequency of surgeries. Such indications of increasing constituency orien-

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"familiar" MP was defined as one with eight or more years' service.

[23] Bear in mind, however, that Curtice and Steed attempt to estimate the *actual* personal vote in their sample of *marginals.* In contrast, our figures again represent the *potential* electoral difference between a very low level of constituency effort and a very high level, averaged across both marginal and nonmarginal districts.

[24] In another article (Cain, Ferejohn, & Fiorina, in press) we show that electoral marginality is significantly associated with an MP's inclination to engage in a variety of constituency-oriented activities. Not surprisingly, those whose fates are more dependent on a few votes more actively seek those votes.

tation have potentially important electoral implications. Whatever the significance of constituency work for electoral behavior today, it might be considerably greater than it was two decades ago, and perhaps considerably less than it might be two decades hence. To explain the importance of that possibly emerging trend, we need to develop a further line of argument.

This article presents a simple cross-sectional analysis based on the static theoretical proposition that single-member district electoral systems provide incentives for candidates to cultivate personal bases of support. This proposition is significant in and of itself, we believe, but it takes on even greater significance when embedded in a dynamic theory of the evolution of party systems. In the United States, for example, a number of scholars have argued that the increasing importance of the personal vote and the decreasing importance of party affiliations are not independent phenomena. Burnham (1971) was probably the first to argue that the decline of party provides fertile ground for the growing importance of personal candidate appeals, which in turn further contribute to the decline of party. The argument is a dynamic one which our data are not adequate to test, but a few moment's consideration will demonstrate its plausibility.

Consider a single-member district system perfectly structured by party. Then by definition there is no personal vote: election outcomes simply reflect the automatic support of partisans for candidates of their party. Now if the hold of party should weaken, perhaps because of poor performance by the party in office, or unpopular issue positions by one or more of the parties, then a *context* is created which did not exist before. With some voters willing to employ decision criteria other than partisanship, the personal characteristics and activities of the candidates might, but not necessarily, begin to take on significance. As Fenno writes (1978, p. 211):

> Incumbency is not an automatic entitlement to a fixed number of votes or percentage points toward re-election. . . . Incumbency should be seen as a resource to be employed; an opportunity to be exploited; and the power of incumbency is whatever each member makes of the resource and the opportunity.

But, assume that some incumbents seize the opportunity and work to build personal bases of support. Then we would expect some feedback effect from their activities: observing hardworking incumbents making special efforts to serve their constituents, still more voters will decide that partisanship should matter less and the individual candidates more, thus further weakening the hold of party. In short, once the dynamic is set in motion it is self-reinforcing; declining parties contribute to increasing personal votes which in turn detract further from the importance of party.

How does the dynamic begin in the first place? Perhaps through some exogenous event(s) as with the aforementioned suggestions of bad performance or unpopular issue stands, or even as a result of more-or-less nonpolitical factors such as a changing media environment, social or technological change, or whatnot. On the other hand an endogenous cause is conceivable. No party system has the kind of complete control posited at the beginning of this argument. There is always some slack in the system. And if incumbents begin to take advantage of such slack—for whatever reason—their resulting electoral benefit may encourage others and thus help weaken the hold of party even in the absence of exogenous disturbances.

What is the upshot of the preceding argument? Are we suggesting that the British party system is undergoing the same sort of decomposition as the American? Not at all. What we are suggesting is that the existence, size, and variation in the personal vote for legislative incumbents is of interest not only for the citizen voting decisions it affects, but also as an important indicator of the strength and trend of a nation's party system.[25] And that is a topic that has aroused considerable interest among British commentators. To complete the argument we need refer to nothing so grand as the current Social Democratic challenge to the established Conservative and Labour parties. Less dramatically, Crewe (1974) describes such trends as the declining share of the vote captured by the two major parties, declining turnout, and increasing interelection volatility in the two-party swing. Proably less significant, but even more interesting from the standpoint of the research described in this article, are the reports of departures in the 1979 results from established patterns of British electoral behavior. Consider some selected remarks of Curtice and Steed in their detailed statistical appendix to the most recent Nuffield election study (Butler & Kavanagh, 1980):

> The 1974-1979 swing was not uniform: it varied more from seat to seat than in any other election since 1950 (1980, p. 394).

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[25] Of course, happenings in the electoral arena are not the only indicator of the state of the party system. The importance of party in the decisionmaking arena (i.e., Parliament) is of equal if not greater importance, although the two arenas undoubtedly have some connection to each other. In this regard an interesting topic for future research is whether the observations of Norton (1980) and Schwartz (1980) on the 1970s weakening of parliamentary party cohesion bear any relation to the concerns of this article.

> It is clear that Labour kept down the swing in its marginal constituencies, particularly in those with less than a 2% two-party majority. . . . A major reason for the low swing, particularly the very low swing in the most marginal seats, is the effect of a change in incumbent MP since 1974. Because of the greater attention he can command in the media and the constituency services he can render, an incumbent MP is more likely to be able to establish a personal vote, consisting of those who support him as an individual rather than as a party representative. Where an MP does build such a personal vote in his favour, that vote will be lost if he is defeated. If he does lose, by the time of the next election the new incumbent MP may have acquired his own personal vote. The combined effect of these two personal votes would be a lower swing against the second incumbent at the following election. . . .
>
> These 18 clear cases amount to strong evidence of the personal vote that an MP can build up. The low swing in them is consistent and appears to be independent of location or type of constituency. For the period from 1974 to 1979, it would appear that the double effect amounted to around 1500 votes in an average sized constituency. . . . It is, of course, in marginal seats that MPs have the greatest incentive to work for such personal votes (1980, pp. 408-409).
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> The more important and unexpected change is the reduction in the number of marginal constituencies. The figures in Table 13 show that, on average, about 12 seats would change hands for each 1% swing. However, the equivalent tables produced after the 1964 and 1966 elections showed that about 18 seats would change hands for each 1% swing. This dramatic reduction in the number of seats liable to change hands has undermined the "cube law," which if it holds, does result in practice in about 18 seats changing hands for each 1% swing (1980, pp. 428-429).

Are the 1979 results aberrant? Apparently not. In a later analysis Curtice and Steed (1982, pp. 268-269) view the 1977 results as the continuation of trends which in retrospect began several elections earlier:

> In 1955 the preconditions for the operation of the cube law were still largely met. . . . Between 1955 and 1970 not a great deal of change occurred, except that there was some evidence of a small secular increase in the standard deviation. . . . In the 1970s both major parties won more seats by large two-party majorities: the distribution of the two-party vote widened and flattened. Before 1974 it was unimodal with a peak near its centre; the distribution is now bimodal with peaks where both parties win safe seats by moderately safe majorities. As a consequence of these changes, the exaggerative power of Britain's electoral system has been systematically reduced in the last three elections.

Nonuniform national swings? Incumbency effects? Vanishing marginals? Decling swing ratios? The American student of Burnham, Erikson, Mayhew, and Tufte should be forgiven a sudden rush of déjà vu, although the magnitudes of the changes discussed by Curtice and Steed are but a pale reflection of those observed in American congressional elections. The changes are noteworthy enough, however, that scholars should not blithely dismiss MPs' activities and their associated personal votes. Whatever its current importance, the personal vote may indicate the condition of important features of the larger electoral system and presage future alterations in that system.

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Bureaucrats and Politicians in Policymaking: The Case of Japan

MICHIO MURAMATSU
Kyoto University

ELLIS S. KRAUSS
Western Washington University

This article extends the recent empirical work on the perceptions and role of bureaucrats and politicians in policymaking. The question of the relationship between politicians and bureaucrats and the role of each in policymaking is especially important in the case of Japan, because the prevalent models of Japanese politics and policymaking are those of the "bureaucracy dominant" or of a closely interwoven "ruling triad" of bureaucracy, big business, and the governing Liberal Democratic Party.

Data are from a systematic survey of 251 higher civil servants and 101 members of the government and opposition parties in the House of Representatives, supplemented by data from other surveys and, wherever possible, compared to equivalent data from western democracies.

The results indicate that Japanese politicians and bureaucrats resemble Western European elites both in social background and in the fact that although the roles of politician and bureaucrat are converging, there are still differences in their contributions to the policymaking process. However, politicians influence policymaking more than most models of Japanese politics have posited, and even government and opposition politicians share some consensus about the most important policy issues facing Japan. A factor analysis demonstrated that higher civil servants' orientations toward their roles vary significantly with their positions in the administrative hierarchy.

The 27-year incumbency of the LDP as ruling party has been particularly important in determining the Japanese variant of the relationship between politicians and bureaucrats. We suggest that the Japanese case shows that the bureaucracy's increasing role in policymaking is universal; however, in late-modernizing political systems like Japan's, where the bureaucracy has always been a dominant actor, the growing power of politicians in postwar politics has been the most significant actor in bringing about more convergence in the two elites. Our data on this trend argue for a more complicated and pluralistic view of Japanese policymaking than that provided by either the bureaucracy-dominant or the ruling-triad model.

In the last few years, political scientists have discovered the bureaucracy and bureaucrats as political actors. An expanding literature has focused, not just on the classic questions of public administration, but on the power of bureaucracy and its specific role and influence in the democratic political systems of advanced industrial societies (for example, Crozier, 1964; Dogan, 1975; Suleiman, 1974). Beginning with Weber's hypothesis that the two greatest shapers of the modern state would be the professional politician and the professional civil servant, a growing subfield has been concerned specifically with their interaction, with the relative degree of their influence, and with their conflict or convergence in the policymaking process. Using systematic sampling and survey techniques, analysts have investigated the related issues of the self-perceptions of bureaucrats toward their role in policymaking and politics, of the personal and professional backgrounds of politicians and bureaucrats, and of the perceptions and attitudes of politicians and bureaucrats toward each other.

The results of this research have been rich but varied. In terms of *what* politicians and bureaucrats do—their roles in policymaking and whether or not they engage in politics—the latest comprehensive comparative study by Aberbach, Putnam,

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and Rockman (1981) indicates that the Weberian ideal distinction between politicians who make policy and bureaucrats who merely implement it does not realistically describe their roles in modern democracies. Rather, these authors find that both engage heavily in politics, and each understands that the other plays a role in policymaking. This convergence of the two elite groups may well result from the increasing complexity and overlapping of functions in the modern state on the one hand, and the political requirements of operating in democratic systems with ever-increasing societal demands from enfranchised citizens and pressure groups on the other.

Yet the proposition that the roles have converged completely is also rejected: if some functions, such as formulating policy and brokering interests, have come to be shared by both politicians and bureaucrats, others still tend to be the preserve of one or the other elite. Bureaucrats still tend to dominate policy and implementation, whereas politicians still tend to be the articulators of ideals (Aberbach et al., 1981, p. 239).

Moreover, there are still important differences between who becomes a politician and a bureaucrat, and how they perform their political and policymaking roles. In democracies as diverse as England, Germany, and Italy, bureaucrats typically had different social origins (more homogeneous) and recruitment patterns from those of politicians. Bureaucrats also had different motivations and approaches to their jobs and different modes of policy analysis, which stem in part from the fundamental natures of their roles. "Politicians and bureaucrats both do policy analysis, but in important respects they do it quite differently" (Putnam, 1975, p. 193).

Aberbach et al. (1981, pp. 9, 24) summarize the main differences; first, "whereas politicians articulate broad and diffuse interests of unorganized individuals, bureaucrats mediate narrow, focused interests of organized clienteles." Second, politicians bring "energy" to policymaking, being partisan and idealistic advocates in style, whereas bureaucrats provide "equilibrium," seeking to avoid conflict, seeking technical and incremental solutions and agreements on narrowly defined problems.

If generally in the Western democracies politicians and bureaucrats are converging somewhat in their roles in policymaking and politics, but not in their style of performing those roles, there are also important variations across nations. Thus, the United States is a consistent aberration, with American congressmen and civil servants somewhat more similar in social backgrounds and much more alike in role focus than are politicians and bureaucrats in Western Europe (Aberbach et al., 1981, pp. 94-100). There are variations even in the Western European nations where politicians and bureaucrats differ in background and policy approach. For example, Putnam (1973, pp. 110-120) has shown that despite the similar parliamentary systems, recent historical experiences, and very common social origins, Italian bureaucrats were more likely to hold classical administrative attitudes toward their and the politicians' roles than German bureaucrats, who were much more political and like British civil servants in their pluralistic attitudes toward the policy process.

In this article we raise many of the same issues about the perceptions and roles of bureaucrats and politicians in policymaking in Japan. Using systematic survey data on samples of politicians and bureaucrats, we seek to test whether Japanese politicians and bureaucrats are similar or different in social background, recruitment, role in policymaking, and orientations to policymaking roles, and the consequent degree of conflict or convergence between them. One of our goals is to place the roles of politicians and bureaucrats in Japan into comparative perspective. Another is to use that perspective for insights into politics in Japan. Especially, we wish to discover the nature and sources of the major cleavages among the political elite in contemporary Japanese policymaking.

The Japanese Case: Major Questions

There are a number of significant reasons why the case of Japan is particularly interesting to analysts of the roles of politicians and bureaucrats in policymaking.

First, whereas the Western democracies have been investigated using systematic attitude surveys, there has been no such English language study of the only non-Western democracy in an advanced industrial society. Aberbach et al. (1981, p. 3) emphasize the uniqueness of the division of labor between elected politicians and career bureaucrats, the phenomenon being found primarily in nineteenth- and twentieth-century Europe and North America. Japan is one of those few modern states outside the West which has witnessed such trends.

Second, there is a particular historical dimension to the Japanese case that invites both comparison and contrast to some European states. As in Italy and Germany, the long-term rise of professional politicians and bureaucrats has taken place in the context of great historical discontinuity. All three countries are late nineteenth-century modernizers with strong bureaucratic traditions; all have experienced both the destruction of democracy before World War II and its reinstitution after the war.

Yet the professionalization of political and bureaucratic elites took place within a shorter time period and with even greater discontinuities in Japan than in any European nation. Neither the beginnings of political parties nor a modern bureaucracy were established in Japan until the 1880s, although the foundations of bureaucratic organization and ethos were laid much earlier, in the Tokugawa period (1603-1868) (Najita, 1974, pp. 16-42). A parliament was not instituted until 1890. Even these institutions differed significantly from their Western European counterparts. Political parties never attained a real mass organizational base, and the principle of Cabinet responsibility to the Diet was never institutionalized in the prewar period, even if the practice emerged for a brief period in the 1920s. Universal manhood suffrage was not granted until 1926, and women were disenfranchized until 1947.

The bureaucracy was "modern" and professionalized in the late nineteenth and early twentieth centuries, but never in the context of party control over the state apparatus. Until the American occupation of Japan, bureaucrats were technically servants of the Emperor (which in fact meant of civilian or military leaders of the Cabinet who except for very brief periods were not party politicians). It was not until the postwar reforms that the context existed to develop a truly professionalized political elite that had access to the levers of state power and a professionalized administrative elite that had to contend with party and parliamentary institutions and politicians.

Thus, just as in the economic sphere, where Japan began to catch up to the West before the war, but the modernization reached fruition only in the postwar period, politically too, elements of the modern state existed before the war but the full institutionalization of the two political elites in the context of universal suffrage and a democratic framework had to await the postwar era. One key question, of course, is whether this telescoping of the stages of state modernization and the very recent institutionalization of a modern democratic framework has created similar or different patterns of politician-bureaucrat interaction than in the West. Another is whether the postwar democratic reforms have resulted in a bureaucracy that accepts the pluralistic rules of the democratic game, as seems to have happened in West Germany, or whether the prewar patterns of classical bureaucratic norms have remained entrenched, as they seem to have done in Italy (Putnam, 1973, pp. 110-120).

The third major reason for exploring Japan as a case of politician-bureaucrat interaction is that the question of the relationship and relative power of bureaucrats and politicians in policymaking has been at the heart of most interpretations of postwar Japanese politics. The two most prevalent and influential models of Japanese politics among both Japanese and American political scientists have been the bureaucracy-dominant and the ruling triad models, although which has been the major and which the minor chord seems to have varied in the two countries.

Among Japanese political scientists, the view that the bureaucracy still has predominant influence in politics is a common one (Ito, 1980; Misawa, 1967; Tsuji, 1969). Misawa (1967) thus analyzes the interaction between politicians and bureaucrats and recognizes the influence of party politicians but ultimately argues for the bureaucracy-dominant theory. Ishida (1960) has implied that many pressure groups are subordinate to bureaucratic influence, and in extreme cases it has even been argued that both the ruling Liberal Democratic Party (LDP) and interest groups can be viewed as mere dependents of the bureaucracy. The bureaucracy-dominant model of Japanese politics has influenced Japanese political science for thirty years, with the causes of the bureaucracy's superior power claimed to lie in the historical development of Japan directly from absolutism to the contemporary administrative state, skipping the stage of citizen society (Tsuji, 1969).

Some American political scientists too have argued for other reasons that the bureaucracy dominates policymaking. First, it has been pointed out that the higher civil service not only survived the Occupation reforms more intact than any other prewar political and social institution, but actually increased its power under the Occupation's policies of economic recovery and post-occupation government's priorities for rapid economic growth (Johnson, 1975, pp. 14-22; 1982, pp. 44-46).

Second, the bureaucracy has seemed to dominate the legislative process to the extent that the Japanese Diet has appeared as merely the ratifier of bureaucratic policy. Most government bills (the overwhelming majority of those passed by the Diet) are initiated by bureaucrats in the various ministries. Additionally, bureaucrats are involved throughout the legislative process. Because of their control over information and technical expertise, they are consulted by the ruling party in their decisions on policy and legislation and are invited to Diet committees to defend drafts of proposed bills. Bureaucrats or ex-bureaucrats even dominate the membership and staffs of the many deliberative and advisory councils that have become important in the policymaking process. The ruling party itself is closely intertwined with the higher civil service. The LDP was formed in 1955 and has continuously been the single, majority government party. During this entire period, ex-bureaucrats (*kanryō seijika*) have constituted at least 30% of the LDP membership in both houses of the Diet and have been seen as more influential in the party than the "pure politician" group (*tōjinha*). The great majority of Prime Ministers have also been ex-officials (see Pempel, 1974, 1982). Finally, it also has been argued that, whatever the role of the bureaucracy in the formal policymaking process, it also actually makes policy through the use of "administrative guidance," bureaucratic ordinances, directives, and informal persuasion (Kaplan, 1972; Pempel, 1974, pp. 654-656). For all these reasons when the question, "Who governs postwar Japan?" has been asked, at least one political scientist has answered directly—"the bureaucracy" (Johnson, 1975).

The second major model of postwar Japanese politics, one with somewhat more influence in the United States than in Japan, is the ruling triad model. Here, the bureaucracy is still one of the major policymaking actors, but is joined by the governing LDP and big business. In this conception, big business supplies most of the political funds to the LDP in exchange for that party passing pro-business policy in the Diet (Yanaga, 1968). The bureaucracy works closely with the LDP and big business to assure conservative policies, and higher bureaucrats retire young to positions as LDP Diet members or as members of the boards of big business. Connected by these vested interests, the same elite social backgrounds, and an old-boy network of school and marriage ties, they share the same values and policy priorities. In sum, "the nation is governed jointly by organized business, the party government, and the administrative bureaucracy" (Yanaga, 1968, p. 27) which constitute a homogenous, interwoven conservative elite that by itself determines national policy (Tsurutani, 1977, pp. 70-116; Yanaga, 1968). They are seen not just as the major elements in Japanese politics, but *as* Japanese politics and as *the* policymakers. The opposition parties, interest groups other than big business (and to a lesser extent agriculture), and the Diet itself are seen as powerless and irrelevant to policymaking.

The ruling triad model has attracted only a few adherents among Japanese political scientists (e.g., Taguchi, 1969), although in a more popular and extreme form it becomes the power elite view of Japanese politics held by, among others, many journalists and intellectuals and some opposition parties. Interestingly, a close cousin of this view is shared by many of a different ideological persuasion, namely the image of "Japan, Incorporated" perceived by many businessmen and journalists on the American side of the Pacific.

Despite the existence of a minority who saw Japanese politics as more complicated and pluralistic (Curtis, 1975; Ike, 1978, pp. 58-63; Reischauer, 1977, pp. 286-297), the bureaucracy-dominant and ruling triad models have been pervasive and have formed much of the basis for discussions of postwar politics. Yet in relation to the comparative literature on bureaucrats and politicians, they have significant weaknesses. First, they are concerned primarily with the question of the relative power of bureaucrats and politicians or their presumed common interests and backgrounds and not with the question of their specific roles (and role conflicts) in, and perceptions of, policymaking. Consequently, many of the questions so salient in the comparative literature have not been analyzed with respect to Japan.

Another problem with both models is that they usually deal only with higher level bureaucrats and the LDP. Thus, middle-level civil servants and opposition party politicians are not taken into account. Yet, to form a complete picture of the relationship between politicians and bureaucrats and the potential sources of cleavages among the Japanese political elite, these groups too must be considered. Most important, the evidence for the models is not based on systematic data on how the participants themselves view each other, their roles, or politics and policy. Observation of the operation of a particular ministry, data on the formal role of bureaucrats in policymaking, and general speculation on the common orientation of policy actors must be supplemented, even confirmed, by evidence of the informal attitudes and norms of the actors involved.

Below we present data from a systematic survey of a sample of Japanese higher civil servants and politicians, both middle and higher bureaucrats, and LDP and opposition politicians. The aim is to consider the related questions of the relative influence of politicians and bureaucrats, of their collaboration or contention in the policy process, and of the roles of these two elites in comparative perspective.

Sample and Methodology

Our data are based largely on elite surveys of bureaucrats and politicians conceived and analyzed by one of the authors and administered in two stages during the period from November, 1976 to March, 1978 by one of Japan's top professional survey research organizations. The bureaucrat sample consisted of 251 higher civil servants. First, eight of the most important bureaucratic organs of the Japanese government were selected: the Ministries of Finance, International Trade and Industry, Agriculture, Forestry and Fisheries, Health, Construction, Labor, and Home Affairs, as well as the Economic Planning Agency of the

Prime Minister's office. Second, six of the eight administrative vice-ministers (the highest career post in the Japanese civil service) and forty-nine bureau chiefs were interviewed to represent the "higher bureaucrats." Then, "the middle-level bureaucrats" were selected from among the same organs' section chiefs and councillors (*shingikan*). These posts are often steps on the way to advancement to the higher posts and represent what have been called the "high flyers" in studies of European bureaucracies (e.g., Putnam, 1973).

The politician sample consisted of 101 members of the 511-member House of Representatives, the most important chamber in the bicameral Diet. The sampling method ensured a representative sample of Diet members in terms of party affiliation and number of times elected. Thus, at the time of the survey, the ruling Liberal Democratic Party held a bare majority in the House, and in our sample 50 of the respondents were LDP members and 50 were members of the five opposition parties, who were also divided in proportion to their strength in the House at that time. For further details on sampling and samples see Muramatsu (1981, pp. 25-28).

The survey's questions concentrated on social background of the elites, career patterns, and attitudes toward each other and toward policymaking and policy. Cross-national surveys by Aberbach et al. (1981), however, also have tested for bureaucrats' perceptions and evaluation of pluralist policy and social conflict. Thus, to supplement our own survey in this area we will also report the findings of a survey of Japanese bureaucrats and politicians conducted by Akira Kubota using questions comparable to the Aberbach et al. study. Kubota's data, which have been published extensively in Japanese but not in English (Kubota, 1977, 1978-1979), will also be used to confirm and check our own findings where similar attitudes are being tested.

Data

Social Backgrounds and Recruitment

First, we shall summarize briefly our findings on the social backgrounds of the politician and bureaucrat samples. Our data showed that bureaucrats come from families with more educated fathers than did politicians, having a higher proportion of both higher and middle-educated fathers. Indeed, almost half the sample of politicians' fathers had only an elementary education, whereas nearly half the bureaucrats' fathers had university backgrounds.

In terms of occupation, we found that the highest percentage of both politicians and bureaucrats (49% and 40%, respectively) had fathers in some public or private managerial position. But there are major differences between the samples. Whereas nearly one-quarter of the bureaucrats had fathers in the professions, only one in ten of the politicians did. If we combine all white-collar and professional occupations except management, more than twice as many bureaucrats (45%) come from this type of social background as do politicians (21%).

A further difference between representatives and civil servants, and a sign of significant inbreeding in the two political elites, is provided by their responses to questions about whether they had any relatives who were bureaucrats or politicians. More bureaucrats (43%) had a bureaucrat in their family than did politicians (30%), whereas many more politicians had a politician in their family (50%) than bureaucrats did (18%). The tendency of Japanese politicians to come from political families has become striking in recent years, especially within the Liberal Democratic Party (Pempel, 1982). This trend, combined with the homogeneous and administrative family background of bureaucrats, may well indicate the increasing social institutionalization of the two political elites.

In terms of region of birth, bureaucrats and politicians diverge, with the latter coming from a wide variety of areas, and the former hailing disproportionally from Tokyo (37%) and the surrounding Kanto area (11%). The same result is found for the location of bureaucrat's high schools: almost 38% received their secondary education in Tokyo.

The divergence between politicians and bureaucrats is even greater when we look at their own educational background. Eighty-five percent of civil servants went to the two most prestigious national universities in Japan, Tokyo University and Kyoto University. Tokyo University especially has been the training ground for elite government bureaucrats since its establishment in the late nineteenth century, and today it continues its overwhelming predominance as the supplier of the nation's best and brightest to the state administration. By contrast, only about one in five of the politicians was graduated from one of these universities, but nearly a third went to a private university and another 30% attended two-year institutions or had no university education at all.

Our results on Japan are strikingly similar to the data from Western European countries. As is shown in Table 1, bureaucrats in all the advanced industrial democracies tend to come from managerial or professional families with fathers who had intermediate or university educations, and to have been educated at a university themselves. Further, just as a disproportionate percentage of Japanese bureaucrats tended to come from the capital region and be educated there, Suleiman (1974, p. 64) found the same was true for French higher civil servants and Aberbach et al. (1981, p. 66) report a similar finding for British bureaucrats. Further, one-third to one-half of the bureaucrats in Britain, Germany, and Italy had relatives in the civil service (Putnam, 1975, p. 97), as in the Japanese case. In both Japan and Western Europe, bureaucrats and politicians both have elite social origins, but the civil servants derive from even more homogeneous and elite backgrounds than the elected representatives.

Incidentally, the differences between bureaucrats and politicians in Japan remain, although at a slightly reduced level in some cases, even when we separate LDP politicians and opposition politicians. Thus, the bureaucrats still have a tendency to come from families with more highly educated fathers and with more fathers in the professions compared with LDP politicians, and in general there is not much difference between LDP and opposition politicians in terms of father's occupation. More LDP politicians (approximately 30%) than opposition politicians went to Tokyo University, but still nowhere near as many as the bureaucrats. One element of the ruling triad model, therefore, that conservative politicians and bureaucrats come from the same elite social backgrounds, is only partially confirmed. There are some differences between the two groups, differences similar to those found between politicians and bureaucrats in the Western democracies.

In Japan, the recruitment and career advancement norms further reinforce the homogeneity of the elite civil servant. Our survey found that two-thirds of the bureaucrat sample majored in law while at university. Also, because all bureaucrats are recruited directly from university and because the Japanese bureaucracy follows rather strict seniority norms in career advancement, there was little difference across ministries in average age at time of recruitment to the civil service or at time of promotion to section or bureau chief level.

Overall, we find the Japanese bureaucrat to come from elite family and educational backgrounds and to be recruited and promoted according to norms that reinforce the homogeneous nature of the group.

General Attitudes Toward Politics, Decision Making, and Society

Differentiation in social background alone does not necessarily guarantee a potential for conflict between political and administrative elites. Such latent conflict requires that these social and career backgrounds produce divergence in perception and attitude as well. The key question is whether bureaucrats from such homogeneous social backgrounds, recruited according to such stringent standards and nurtured in the environment of an

Table 1. Comparison of Social Backgrounds of Politicians and Bureaucrats in Western Europe and Japan

	Bureaucrats					Politicians				
	It	Ger	Fr	Br	Ja	It	Ger	Fr	Br	Ja
Father's occupation[a]										
Managerial or professional	82	67	96	68	64	41	51	76	64	59
Other nonmanual	16	21	3	21	21	27	20	18	12	11
Manual	3	12	1	13	7	33	29	6	25	27
Father's education[b]										
None or primary	13	36	–	29	21	–	–	–	–	48
Intermediate	52	34	–	43	30	–	–	–	–	14
University	35	30	–	27	45	–	–	–	–	31
Education[c]										
University	100	99	97	83	98	84	67	79	71	67
Less than university	0	1	3	16	2[d]	16	33	21	29	27[d]

European samples do not always sum up to 100% due to rounding; Japanese samples do not always add to 100% owing to NA and DK responses. Blanks indicate no data were available.

[a] European data source: recalculated from Aberbach et al. (1981, p. 55).

[b] European data source: recalculated from Putnam (1975, p. 97).

[c] European data source: based on Aberbach et al. (1981, p. 49).

[d] Includes two-year colleges and technical schools.

Table 2. Bureaucrats' Attitudes toward Pluralism (% agreeing)

	Japan	It	Br	Ger
1) Interest groups' conflict endanger country's welfare	69	88	18	19
2) Political parties exacerbate conflict	96	85	54	28
3) Civil service guarantees reasonable public policy	80	–	21	16
4) Technical considerations should outweigh political	49	77	21	50

Japan percentages include those who "agreed" and those who "agreed somewhat." Percentages that were transposed from Kubota's figures are approximate to within 1 to 3 percentage points.

Source: Kubota (Kankai 3/78, pp. 107-110; Chūō Kōron, 1977, p. 194); Putnam, 1975, p. 101.

elite administrative corps, bring to their role attitudes consonant with the requirements of pluralist politics in democratic systems. Such backgrounds may well produce classical bureaucrats, who see problems amenable to solution by nonpartisan and objective analysis and thus who downgrade the role of parties, parliaments, and pressure groups in decisionmaking. Studies of European bureaucrats indicate that Italian civil servants fall close to this end of the continuum of bureaucratic types. But this need not inevitably be the case. The advance of democratic ideals in the postwar era, the rise of new generations within the bureaucracy more committed to those ideals than older generations, and over 35 years of experience with pluralist political systems may well produce bureaucrats who at least accept and at best are committed to political pluralism. Such seems to be the case of the West German administrative elite (Putnam, 1973).

What about Japan? Do Japanese bureaucrats with their highly elite and homogeneous background and career environments favor the norms and perceptions of rational bureaucracy over the messier world of pluralistic politics? Or have they come to accept politics, politicians, pressure groups, and conflict as a normal, even desirable, part of governance?

Lacking data from our own survey to answer these questions, we turned to results reported by Akira Kubota (1977, 1978-1979). He administered a series of questions identical to those asked of European bureaucrats by the Aberbach et al. (1981) study, and which the latter used as part of an index of political pluralism. The question wording was as follows: 1) The general welfare of the country is seriously endangered by the continual clash of particularistic interest groups; 2) Although parties play an important role in democracy, they often uselessly exacerbate political conflicts; 3) Basically, it is not the parties and parliament, but rather the civil service that guarantees reasonably satisfactory public policy in this country; 4) in contemporary social and economic affairs it is essential that technical considerations be given more weight than political factors. Agreement with these items would indicate bureaucrats who had relatively less tolerance for pluralistic politics and a strictly classical, administrative approach to policymaking. The results for Japan, along with comparative data on Italy, Germany, and England where available, are presented in Table 2.

The findings indicated that Japanese bureaucrats, both absolutely and comparatively, score rather highly in negative attitudes toward pluralism. Almost all of the Japanese civil servants believe that political parties often needlessly intensify political conflicts, and a large majority of them also believe that the clash of interest groups seriously endangers the national welfare. Further, they believe that it is they, not the parties or legislature, who ensure reasonable public policy and that technical factors should have precedence over political ones. Comparatively, Japanese bureaucrats are relatively more like the classical bureaucrats of Italy than the more pluralistic bureaucrats of Britain and Germany.

Kubota also reports the evaluations of his Japanese bureaucrat and politician respondents and their European counterparts concerning the extent of conflict that exists among groups in their societies. His data show that a larger percentage of Japanese bureaucrats (58%) view society as harmonious than do Japanese politicians (35%). Even on a comparative scale, Japanese bureaucrats see the relationship among groups in society as much less conflictual than do the bureaucrats of any other nationality. About a third of Japanese politicians see social group relations as harmonious and about 40% as conflictual, which is more or less in line with their European counterparts, but Japanese bureaucrats are twice as likely or more to see society as harmonious than their European colleagues (58% to a range of 15-29% for the four European countries).

Kubota's sample differs from ours,[1] so we cannot say with complete certainty that the bureaucrats in our survey would have responded similarly. Based on his data, however, it appears that the Japanese civil servant's view of politics and society is less pluralistic than either the Japanese politician or most European bureaucrats (except perhaps the Italian). He is likely to approach his task with a fear that social interest groups and political parties create conflict, yet a strong tendency to see his own society as having few conflicts. He also tends to believe that it is the bureaucracy and a technical approach to problem solving that will benefit the public interest. A modern tradition of bureaucratic dominance, the collapsing of historical phases of modernization, the relatively recent implementation of political party democracy, and elitist background, recruitment, and career factors seem to have left their mark on the attitudes of Japanese bureaucrats (see also Craig, 1975, pp. 4-11).

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[1] Among other differences, Kubota's sample was drawn from four other ministries in addition to the ones

In general attitudes toward politics, Japanese bureaucrats' perceptions are in accord with the bureaucracy-dominant model. It remains to be seen whether this also applies to the attitudes they bring to their specific roles.

Policymaking Roles

Even if Japanese civil servants have different backgrounds from those of politicians and lean toward the classical bureaucratic approach in their general attitudes about society and the political process, they may or may not differ from politicians in their specific evaluations of the proper and actual role of the two groups in the policymaking process. Current role experiences can reinforce the divergent backgrounds and general attitudes that exist between politicians and bureaucrats, or they can mitigate those differences to produce elite groups who view the process and content of policy similarly. Or the current role variables and experiences can outweigh other factors, producing different and more important lines of division, for example, between those with greater responsibility, such as higher bureaucrats and ruling party politicians, and those with less responsibility for policy, the middle bureaucrats and opposition politicians. In the next sections we will look at the more concrete orientations of how political elites perceive their roles, their influence in policymaking, and policy issues. In effect, we will be testing for whether social backgrounds and general attitudes, institutional roles, or organizational responsibilities produce the lines of greatest cleavage among the political elites. In order to do this, we will want to subdivide further our samples into higher and middle level bureaucrats and LDP and opposition politicians.

In their recent study of European and American bureaucrats, Aberbach et al. (1981, pp. 4-21) described four possible types (what they called "images") of relationships between politicians and bureaucrats. Image I (Policy/Administration) represented the traditional division that politicians make policy and bureaucrats administer it, i.e., that there is a strict division of labor with bureaucrats uninvolved in any aspect of policymaking except its implementation. Image II (Facts/Interest) gives bureaucrats a somewhat greater role in policymaking, but the very specific one of bringing facts and knowledge to the process. The politicians' role is to emphasize responsiveness to interests and constituencies in policy formation. Thus, civil servants provide neutral expertise, whereas politicians make decisions on the basis of values and interests. Image III (Energy/Equilibrium) has both groups heavily involved in both politics and policymaking, but with politicians providing the ideals and energy to the system, whereas bureaucrats emphasize incrementalism and pragmatism. In this type of role division, bureaucrats are the conflict managers, particularly of organized interests, whereas politicians address broader and more controversial issues and articulate unorganized interests. Finally, Image IV (The Pure Hybrid) is one in which both elites' functions and roles have merged: politicians increasingly acquire administrative skills, technical expertise, and bureaucratic orientations, whereas bureaucrats are totally involved in the political and policymaking process, including the setting of the broader directions of policy.

It should be noted that these four images provide a continuum, with each successive type cumulatively adding another policy function to the bureaucracy. Thus, whereas in Image I the bureaucracy only implements policy, in Image II it also helps formulate it (by providing the groundwork for politicians' decisions), in Image III it additionally brokers interests, and in Image IV it articulates ideals as well (Aberbach et al., 1981, pp. 238-239). Each progressive image of this typology also represents the increasing involvement of the civil service in political roles, from neutral implementation, through involvement in decisionmaking and brokering interests, to articulating the ideal goals and direction of the nation.

In the Western democracies study, no single

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represented in our sample: Legal Affairs, Transportation, Posts, and Education. His sample also includes assistant bureau chiefs in addition to the positions in our sample. Finally, Kubota's survey was conducted during a period that only partially overlapped the administration of our survey. See Kubota (2/77, p. 193).

Table 3. Specific Role of Bureaucracy

	Higher Bureaucrats	Middle Bureaucrats	LDP Politicians	Opposition Politicians
	%	%	%	%
Implement Diet and party policy (Image I)	9	10	16	22
Lay groundwork for political decisions (Image II)	44	34	44	39
Coordinate competing interests and views (Image III)	27	40	24	27
Change society in desirable directions (Image IV)	15	15	8	6
NA	5	1	8	6
	100	100	100	100
N	(55)	(196)	(50)	(51)

question measured the four images, but rather these images represented a summary conglomeration of many of the measured attributes. By coincidence, however, our survey, administered in Japan, contained a question whose responses conformed closely to the image types in Aberbach et al.'s study. The question asked respondents what role the bureaucracy was usually performing at present and offered them the choice of "policy implementation that reflected the wishes of the parties and Diet," "laying the groundwork for political decisions on important problems," "coordinating competing interests and views," and "transforming the social structure in desireable directions." This categorization is not a perfect description of the four images and was not designed originally as a continuum. Nevertheless, each of these choices parallels nicely the essence of each of the roles for bureaucracy in the above-described images and can be treated as a continuum. Thus, they provide us with a view of how Japanese politicians and bureaucrats perceive the relationship between civil service and politics in contemporary Japan. The results are presented in Table 3.

There is little support for the perception that the civil service is either purely administrating or implementing political decisions made by parties and parliament (Image I-type), or that it is usurping the role of elected officials to decide the nation's goals and lead the country toward social transformation (Image IV-type). Rather, the bureaucracy is definitely seen as involved in politics and policy, but as a provider of information and technical expertise to the politician (Image II-type) or as a broker and manager of social interests (Image III-type). This result is strikingly similar to those found in Western Europe. Aberbach et al., *found that Image I and Image IV* were rejected by their respondents, whereas Image II was the most popular formal choice, but Image III was probably the most realistic interpretation of their role in practice (Aberbach et al., 1981, p. 241). In both Japan and Western Europe, the role of politicians and bureaucrats has been converging, but there are still differences in their contributions and emphases in the policymaking process.

The partial convergence of the two roles is confirmed in data reported by Kubota (3/79, p. 119). When asked directly whether politicians and bureaucrats were similar or different from each other, the majority of politicians and bureaucrats in all countries respond in terms of moderate similarities or differences rather than that they were "very" or "more or less" similar or that they were completely different.

Turning again to our own survey, although the differences among categories of actor were not great, they are interesting. The politicians are more likely to see the bureaucrats as pure implementors of policy or as aids in its preparation. Bureaucrats themselves are more likely to see their role as political—managing social conflict or leading the way toward desirable social change. We also find some indication of the impact of role position on perceptions: the middle-level bureaucrat diverges from the politicians, and from top bureaucrats, especially in choosing the coordination of competing interests as the main role of the civil service. Thus, we find evidence that although the roles of bureaucrats and politicians in policymaking may be converging—civil servants performing political functions and playing a major role in the policymaking process—there are some differences of perception between politicians and bureaucrats and between higher and middle bureaucrats about what the role of the civil service is in that process.

In contrast to their general attitudes toward politics, when asked about their specific role in policymaking, Japanese bureaucrats do not display particularly bureaucracy-dominant perceptions. Rather, they see their role as aiding politicians in decision making or as conflict managers of social groups. The ruling triad model, on the other hand, predicts only the bureaucracy's connections to both politicians and interest groups in a vague and generally equal relationship; it does not suggest the specific decision-maker role of the politician vis-a-vis the bureaucrat, nor the active coordinator role of the bureaucrat vis-a-vis the businessman. We find that neither of the standard models of Japanese policymaking fits the perceptions that the participants themselves have of their roles.

Influence in Policymaking

If bureaucrats in Japan as elsewhere are playing more of a role in policymaking, albeit in ways still somewhat different from politicians, how much influence do they have in that process? This question, of course, has been central to discussions of the postwar Japanese political system. Do Japanese political elites themselves believe that the bureaucracy dominates policymaking to the extent that some political scientists have described? Or do they see themselves as part of an interconnected elite with politicians and businessmen as in the ruling triad model?

The respondents were asked which group had the most influence in deciding national policy. Table 4 shows the results. There is agreement among the groups that the only influential actors are party politicians and bureaucrats, against whom all other groups' perceived influence pales by comparison. And, except for the LDP representatives, who tend to see party politicians as more influential than bureaucrats by a wide margin, the other groups are all almost equally divided as to which dominates the decisionmaking process of postwar Japan. Given the prevalent view of the great influence of the higher civil service, these are somewhat surprising results. Also surprising is the perceived weak influence of businessmen on policy, given the conceptions of the ruling triad model.

Some might argue that these surprising results are caused by methodological artifact. For example, one could object that LDP politicians would naturally exaggerate their own influence, whereas the Japanese bureaucratic style is to be "self-effacing" about their role in policymaking. Although superficially appealing, these arguments cannot consistently explain the results. For example, Japanese bureaucrats were certainly not self-effacing about the superiority of the bureaucracy in their general attitudes toward politics; why should they be arrogant on one dimension and self-effacing on another? Further, opposition party politicians' responses provide a check on the validity of the other responses: they have every ideological reason to exaggerate the role of business and the bureaucracy in policymaking, and yet they too see politicians as influential as bureaucrats and much more so than businessmen. Finally, in surveys of this kind if there is a bias at all it is for *each group* to exaggerate the influence of the other (Putnam, personal communication), thus mitigating a unidirectional bias.

On the other hand, we ourselves would not necessarily debate that business' influence on policy may be somewhat greater than is reflected in these figures. Undoubtedly, some respondents interpreted the question to mean "*direct* influence." Business may have indirect influence as well through its connections to LDP politicians. Yet the fact that both bureaucrats and politicians, even opposition politicians, attribute far more

Table 4. Group Most Influential in Policy Process

	Higher Bureaucrats	Middle Bureaucrats	LDP Politicians	Opposition Politicians
	%	%	%	%
Party politicians	47	45	68	43
Bureaucrats	46	40	30	41
Judges	*	*	*	*
Businessmen	*	5	*	14
Labor	*	*	*	*
Agriculture	*	3	*	*
Intellectuals, Religious groups & citizens' Groups	*	1	*	*
Mass media	4	4	2	*
Other and NA	4	3	*	3
	101	101	100	101
N	(55)	(195)	(50)	(51)

*None or less than 1%.

Table 5. Influence of Bureaucracy and Diet in Policymaking

	Higher Bureaucrats	Middle Bureaucrats	LDP Politicians	Opposition Politicians
	%	%	%	%
Influence of bureaucracy				
High (18-21)	38	19	66	65
Medium (14-17)	55	58	28	32
Low (0-13)	7	22	6	4
	100	99	100	101
Preference for Bureaucracy's Future Influence				
Desire increase	6	8	2	6
Desire present level	26	32	8	8
Desire decrease	66	55	90	86
Other	2	5	*	*
	100	100	100	100
Influence of Diet				
Quite a Bit; or A Great Deal	84	70	84	81
Not Much	16	27	12	16
Hardly Any	*	4	4	4
	100	101	100	101
N	(55)	(196)	(50)	(51)

*None or less than 1%.

influence to their own groups than to business should make us pause before accepting uncritically the assertions of the ruling triad model.

Nor would we argue that these data show that the bureaucracy's role is at all insignificant. We asked our respondents directly to rank the bureaucracy's influence on a scale from 0 to 7 (0 = none; 7 = very much influence in policy) in three different policy areas (economic, agricultural, and health). Putting these three evaluations together into a composite index, we see in Table 5 that all groups evaluate the bureaucracy as having at least moderate influence. There are two interesting differences between the groups, however. On the one hand, there is a bureaucratic-politician split, with the politicians perceiving the influence of the bureaucracy to be much greater than the bureaucrats themselves see it. On the other hand, within the bureaucracy we also see a difference between higher and middle bureaucrats: higher-level bureaucrats are more than twice as likely than middle-level civil servants to see the bureaucracy as having a very great influence.

We also asked the respondents whether they desired an increase or decrease in discretionary administrative decisions under bureaucratic leadership. Table 5 indicates that none of the categories of politicians or civil servants desires bureaucracy's independent influence to increase; over half prefer to see it decrease. But there are differences between the politicians and the bureaucrats: representatives of both the governing and opposition parties are more likely, and in overwhelming proportions, to prefer that independent bureaucratic decision making decrease in the future. Again, however, higher- and middle-level bureaucrats differ, with top civil servants being closer to the politicians in desiring a decline.

Finally, when asked the extent to which Diet deliberations currently influenced the formation of policy, we note in Table 5 that both higher-level and middle-level bureaucrats and both LDP and opposition politicians believe the Diet does have such power (about two-thirds or more of each category responded "quite a bit"; the proportions answering "a lot" were much smaller). Significantly, on the influence of the Diet on policy, higher-level bureaucrats and LDP and opposition representatives differ hardly at all. The deviant group is the middle-level bureaucrats, who see the Diet as having somewhat less influence than the others. When it comes to evaluating the extent of parliamentary influence on policy, therefore, the bureaucrat-politician split is not as significant as the divergence of the middle-level bureaucrats.

These results are somewhat different and more complicated than those that might have been predicted on the basis of the bureaucracy-dominant themes in the literature on Japanese politics. The findings indicate that although the bureaucracy is a major and powerful actor in the policy process, the political organs of party and parliament are perceived to play a very influential role as well. Further, although there is a tendency for bureaucrats and politicians to disagree about the extent of bureaucratic influence and the desirability of independent bureaucratic power on policy, the extent of that divergence is not great. Finally, again we find that perceptions of higher bureaucrats resemble more closely those of politicians than of middle-level civil servants in estimations of the relative power of actors in the policy process, just as they did in defining of their specific policymaking roles.

Results of one question from Kubota's study (3/78, p. 110) confirm both our earlier results that Japanese bureaucrats tend to have rather negative and classical views of pluralist democracy and decision making and our results indicating that they nonetheless perceive politicians as having great influence in the policymaking process. Among Japanese civil servants, 41% agreed unqualifiedly, and another 36% agreed less strongly, with the statement that "One of the unfavorable tendencies in contemporary politics is for politicians to interfere in problems which ought to be left to the bureaucracy." Comparatively, Japanese bureaucrats resent political "interference" more than the bureaucrats of Germany and England. Only the highly classical bureaucrats of Italy score higher on this measure. Japanese bureaucrats' reactions to this question reinforce our other findings that Japanese politicians have greater influence, and have been increasing it, than most models of Japanese politics have described, and that the homogeneous, elite, and also influential Japanese bureaucracy does not particularly like this new tendency in postwar politics.

The Impact of Organizational Roles and Generations

Before turning to perception of policy issues, we want to explore further the question of whether Japanese bureaucrats are classical or political and to see if part of that answer can be found by testing for the influence of organizational roles and generational age. In terms of general tendencies in our overall sample of bureaucrats, Japanese civil servants, on the one hand, seem to fit the classical bureaucratic type: they are part of a homogeneous, inbred bureaucratic elite who are skeptical about the messy world of pluralistic democratic politics and who resent the increasing tendency of politicians to interfere in policymaking. On the other hand, they also have characteristics of the political bureaucratic type who recognizes the dominance of the party and legislative apparatus in democratic policymaking and who acknowledges as legitimate the demands of social pressure groups in a pluralist society (Putnam, 1973). For example, they perceive parties and parliament to have a major role in policymaking, and even desire to see the independent influence of the bureaucracy diminish in the future. They further define their own role as an adjunct to decisions made by politicians or to be managers of social group conflict. We wanted to see whether this ambivalence in the sample as a whole could be explained by the effects of organizational role, by the effects of generational differences among bureaucrats, or by both.

In considering this seeming paradoxical mixture of classical and political bureaucratic attitudes in the Japanese civil service, we became aware that these concepts themselves may be part of the problem. Indeed, as Putnam (1975, p. 120) has suggested, this dichotomy may be oversimplified. We suspected that the notions of classical and political bureaucrats actually contain, and confuse, at least two conceptually distinct ideas. First, they imply an official can favor bureaucratic leadership or party leadership in policymaking (classical bureaucrats the former, political bureaucrats the latter). But they also imply an official may place priority on defining and advancing the national interests or on managing purely sectarian and social interests (classical bureaucrats the former, political bureaucrats the latter).

Indeed, when we conducted a factor analysis by a method commonly used by Japanese social scientists,[2] using a series of questions on bureaucratic norms from our survey of civil servants,[3] the results showed two separate dimensions, one of national vs. interest-group orientation, and the other of administrative vs. political leadership. With these two dimensions, we find four different types of bureaucrats instead of a simple political vs. administrative distinction: 1) bureaucrats who accept political leadership as dominant but are more oriented toward governing in the "national" interest (the *party government* type); 2) bureaucrats who accept political leadership as dominant and who see their role as managing social interests (the *pluralist* type); 3) bureaucrats who desire administrative leadership but recognize the legitimacy of social interests as long as they are tied to a primarily administrative state (the *corporatist* type); and 4) bureaucrats who desire administrative leadership governing primarily in the name of an overarching national interest (the *classical* type).

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[2] The method, which is closely related to Guttman Scale Analysis, involves a method of classification of individuals based on the similarity of responses to questions having several categories, the simultaneous classification of individuals and categories, and vector analysis. For details in English on this quantification on response pattern: factor analytic method for qualitative data (known as Quantification Theory III-type analysis in Japan), see Hayashi (1982) and Hayashi and Suzuki (1975).

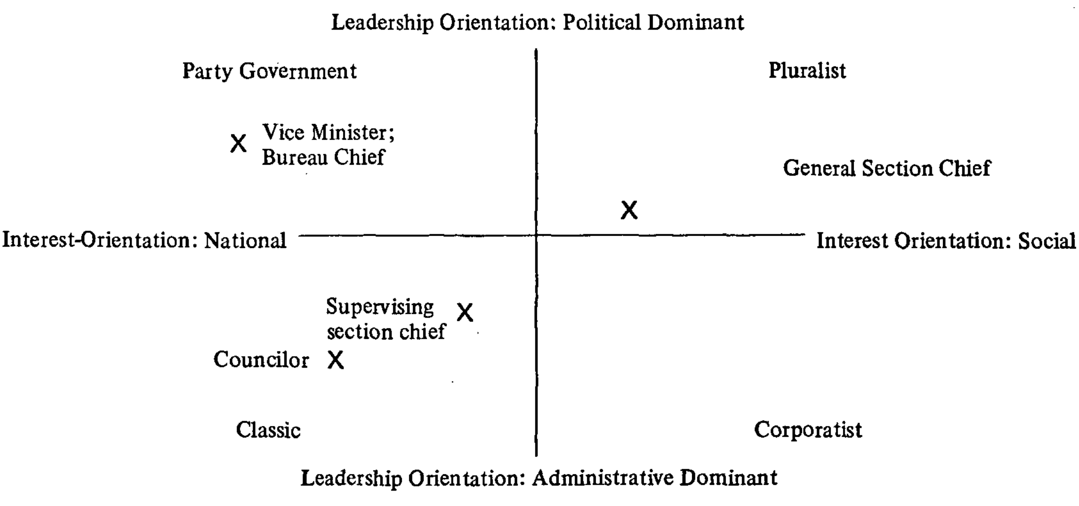
[3] Four questions were used: 1) The question concerning the role of the bureaucracy discussed above and in Table 3; 2) In a case where you were making a discretionary administrative judgment, one that took a high degree of judgment of the type that called for approved office and administrative leadership, what would you take as your main standards: technical stan-

Further, we hypothesized that these types of bureaucrats might vary with position in the organizational hierarchy, in other words, that the specific norms of one's bureaucratic position and task might be related to attitudes toward elite leadership and toward national and social interest. We were particularly interested in testing for this relationship because we have already found a consistent pattern of moderate but distinct differences between higher- and middle-level bureaucrats in Japan. The middle-level bureaucrat does not recognize the influence of the Diet in the formation of policy as much as the higher bureaucrat, but he is more likely than the higher bureaucrat to see his role as the political one of coordinating competing social interests. We therefore factored administrative position into the previous analysis.

Figure 1 gives the results of this factor analysis, placing each bureaucratic role in two-dimensional space on our classification according to their average score on the dimensions. The figure shows that bureaucratic roles are clearly differentiated according to type of bureaucrat. Thus, the vice-ministers and bureau chiefs were firmly in the upper lefthand quadrant of party government types, recognizing both political leadership and oriented toward the national interest. The middle bureaucrats of councillor and supervising section chiefs were in the lower lefthand quadrant of classic bureaucrat (although the supervising section chiefs were closer to the corporatist side than the councillors). Finally, the low-middle bureaucrat, the ordinary section chief, was in the pluralist quadrant.

These results indicate strongly that how classical or political bureaucrats are, including Japanese civil servants' attitudes toward bureaucratic or political leadership and toward governance oriented to national or social group inter-

Figure 1. Types of Bureaucrats


Leadership Orientation: Political Dominant
Party Government
Pluralist
X Vice Minister; Bureau Chief
General Section Chief
X
Interest-Orientation: National
Interest Orientation: Social
Supervising section chief X
Councilor X
Classic
Corporatist
Leadership Orientation: Administrative Dominant

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dards, conformity with policy goals, a balance of interests, the national interest? 3) Supposing you have to persuade your Minister to abandon a policy he was thinking about, what argument do you think would be the most effective: technical difficulties, legal difficulties, the opposition of interest groups? 4) When there is a conflict among the contentions of upper-level public bureaucrats, the opinions of the party to which your minister belonged, and the demands of interest groups with which your minister has contacts, on which do you think your minister places the most priority?

est, is related at least in part to one's position and task in the bureaucratic hierarchy.

Another possibility is that some of our results about the mixture of classical and political attitudes of the Japanese bureaucracy may be explained by generational effects. Putnam (1975) found, for example, that in Germany and Italy, two countries with authoritarian, bureaucratic-dominant states just a generation ago, classical bureaucratic attitudes were more common among those raised in the prewar years and less so among younger bureaucrats. Since Japan has a similar modern history, such a phenomenon might also be present in Japan. When we tested for the impact of generational effects, we found only a mild influence on certain measures, with prewar (older than 48 years of age at the time of the survey) bureaucrats somewhat more likely to emphasize technical expertise and the political aspects of their role and to evaluate their influence in policymaking highly, whereas the postwar generation (under 47 years of age) was more likely to focus their role on the relationship to pluralist interest groups and was less sanguine about bureaucracy's influence. The view that the Diet has a significant influence on policy is not subject to any generational differences, and even older bureaucrats share this view. It would appear that some differences between bureaucratic generations exist, but these are very selective and moderate in impact.[4] Moreover, because of the strict seniority norms in promotion within the Japanese civil service, such generational effects become inextricably bound up with the stronger and wider effects of organizational rank.

Policy Issue Area

Finally, we come to the question of how similar or different bureaucrats are with respect to their perceptions of policy itself. In other words, here we will be concerned with the content of policy rather than the role of the actors in the policymaking process. Perceptions of policy may be strongly influenced by politicians' or bureaucrats' ideological positions. We would expect this particularly in Japan, where during the 1950s and 1960s the country was polarized into conservative and progressive camps, and numerous clashes took place, some violent, between the LDP government and the opposition forces both inside and outside the Diet. The polarization and confrontations took place particularly over such policy issues as national defense and foreign policy, education, and public order. Analysts described Japanese politics during this era as "cultural politics," because at the root of the policy differences were basic value and ideological differences between the two camps over the question of the relationship between the governors and the governed (Kyogoku, 1969; Passin, 1962; Watanuki, 1967).

Some of the vestiges of ideological differences could be seen in a question in the survey which asked their degree of satisfaction or dissatisfaction with present society, and in which we found that opposition party politicians are much more dissatisfied with present arrangements than LDP politicians and top-level bureaucrats.[5] However, at a less abstract level, in recent years ideological differences seem to have moderated, and there is evidence of an emerging center and incipient consensus in Japanese politics. One of the key questions we must raise, therefore, is whether any remaining ideological policy differences between the ruling LDP and bureaucracy on the one hand, and the opposition party politicians on the other, impinge on their evaluations of policy.

One way to test this question is to investigate our respondents' view of the most pressing problems facing contemporary Japan and to ascertain whether they differ systematically in terms of their perceptions of the areas of policy that need the most attention. The samples were asked to name the three most important problems facing Japan in rank order, and their answers were then grouped according to general issue areas. Their responses for the first most important problem chosen are shown in Table 6, which demonstrates a fair degree of unanimity concerning contemporary issues: no category of respondent differs on any issue area from any other by more than 20%. Generally, the pragmatic problems of

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[4] The differences between generations on the question of the role of bureaucracy were neither large nor statistically significant, with large pluralities of both groups agreeing that coordination of interests and providing information to politicians is the prime administrative role. The prewar generation is more likely (32% to 16%) than the younger generation to score highly on our index of bureaucracy's influence in the three areas of policy, is more likely (48% to 35%) to choose bureaucracy as having the most influence of any group in policy. And is also more likely (61% to 45%) to see bureaucratic influence in the near future as remaining the same as at present. One measure on which there was no appreciable difference at all was on the question of the influence of the Diet in policymaking where both age groups by two to one majorities rated its power as considerable or strong.

[5] Only 22% of the higher bureaucrats were "dissatisfied" with contemporary society (the remainder "satisfied" or "average"), with that rising to 40% of the middle bureaucrats and 46% of the LDP politicians, compared to 91% of the opposition politicians.

Table 6. *Bureaucrats' and Politicians' Evaluations of the Most Important Problem Facing Japan*

	Higher Bureaucrats	Middle Bureaucrats	LDP Politicians	Opposition Politicians
	%	%	%	%
Energy; resources	22	13	26	27
Reform of education	18	14	16	6
Search for new values; correcting ills of modern society	24	24	26	37
Search for new state-society relations; new decisions in public policy	16	29	10	12
Security; international relations	13	13	18	12
Other	7	8	4	6
	100	101	100	100
N	(55)	(196)	(50)	(51)

energy and resources, security and international relations, and the value problem of dissatisfaction with modern society are the most frequently chosen by most groups.

But within this general consensus, there are important variations. The two groups that most closely resemble each other are higher bureaucrats and LDP politicians. The percentage of each of these groups is nearly exactly alike on all the issue areas, and the relative order of the alternatives chosen is very similar. Both middle-level bureaucrats and opposition politicians deviate from the consensus within the party and administrative governing elites, but in different ways. Opposition politicians are more likely to choose the problem of the values and defects of modern society than any other group. Given the ideological differences of many opposition politicians with those who govern, and their dissatisfaction with the status quo in Japan, this is perhaps not surprising.

Middle-level bureaucrats also differ from both the governing party and their own superiors in the bureaucracy. These bureaucrats tend to see state-citizen relations and public policy itself as more of an issue than the other groups, including the opposition parties. They are less impressed with energy and resources as a pressing issue. Once more we see that there are differences in the perceptions of middle-level and top civil servants in Japan, a finding for which neither the bureaucracy-dominant or ruling triad model would have prepared us. We shall discuss some of the possible reasons for this consistent tendency below.

Conflict and Consensus among Political Elites

Our data demonstrate a complex pattern of relationships between politicians and bureaucrats in policymaking, a pattern organized along three important cleavages among Japanese elites: the institutional, the organizational, and the political.

First, we find a mixed pattern on the institutional cleavage of politicians versus bureaucrats. As in Western Europe, politicians and bureaucrats are converging in the sense that both play important roles in the policymaking process, and both recognize the role the other plays in that process. The bureaucrat does not simply administer policies decided upon by the politicians; he also plays a political role in helping to formulate policy with the politicians by providing expertise and knowledge and by managing conflict among interest groups.

Also as in Western Europe, however, the two roles still diverge. Neither most civil servants nor politicians believe that the bureaucracy should provide the overall goals and ideals for policymaking; that is the predominant preserve of the politician. Furthermore, politicians and bureaucrats differ greatly in their social backgrounds and general attitudes toward politics and society. Indeed, we have found a deeper cleavage between bureaucrats and politicians on these dimensions than in most Western European democracies except for Italy. Japanese bureaucrats constitute a very homogeneous and prestigious elite compared to politicians and hold elitist attitudes toward politicians and pluralist democratic politics. Resenting interference in what they perceive as bureaucratic prerogatives in policymaking, they approach policymaking with the classical administrative style of bureaucratic manager, believing that social harmony is primary and that technocratic and accommodative decisions made by experts such as themselves can best guarantee good policy. When it comes to their conception of their institutional roles, therefore, we find much evidence for latent conflict between the two elites, despite their convergence in some respects.

Yet this potential institutional conflict is greatly mitigated by other cleavages that cut across it. One of these is the organizational cleavage within the ranks of the bureaucracy itself: we have found consistent evidence that top bureaucrats are more likely to acknowledge the power of politicians and parliament in policymaking and to perceive the salience of policy issues more like LDP politicians, but are less likely to conceive of the bureaucracy's role as managing social interests than middle-level bureaucrats. We believe there are several explanations for this pattern of cleavage between highest- and middle-ranking civil servants.

One explanation is the difference in their organizational tasks. The finer distinctions we have made between types of bureaucrats and the results of our factor analysis (Figure 1) indicate strongly the importance of the role norms of a particular level of post in the bureaucratic hierarchy in determining perspectives on the policymaking process. It would not be an exaggeration to use the old adage that where one stands depends on where one sits. The perspective of the higher bureaucrat (vice-minister, bureau chief) is political in the sense that he is the civil servant who must work most closely with LDP politicians and the Diet. He therefore recognizes most clearly the infuence of parties and parliament on the policies ultimately produced, and the realistic constraints that such factors place on the power of the bureaucracy. Consequently, among all the types of civil servants, he is most likely to see the bureaucracy's role as one of laying the groundwork for the major decisions to be made by politicians and to recognize the actual power of politicians and parliament in policymaking.

The ordinary section chief is the lowest middle-level civil servant. Closest to the daily operations of the Ministry, he is most likely to feel the pressures from the interest groups from politicians' constituencies who desire changes in policy or dispensation in its implementation that favor their group. He therefore sees the role of bureaucracy primarily as coordinating pluralist interest politics and recognizes the importance of party and politicians in satisfying those interests. But he is also furthest from the high politics of the party and parliamentary leadership and thus has much less of a conception of the constraints and power of the Diet and politician in formulating and deciding on policy.

The mid-middle bureaucrats (councillors and supervising section chiefs) are in-between in more than just formal position. They are somewhat insulated both from the grassroots political pressures of interest groups and the higher bureaucrats' problems of reconciling party and parliamentary constraints with rational policy. In stage of career, they literally are organization men, primarily concerned with problems of organizational coordination and efficiency. Especially, councillors are often called upon to participate in the Chief Cabinet Secretary's attempts to bring about inter-ministerial consensus on policy, a task in which a national viewpoint on policy is emphasized, but at the same time the councillors usually are expected to represent the interests of their own ministries (Campbell, in press). Involved most actively in bureaucratic politics per se, they (perhaps naively) come to believe that administrative leadership in the national interest is both reality and preferable.

Reinforcing differences in role perspectives is one simple fact of political life in Japan: the Liberal Democratic Party has been the sole party in power for about twenty-seven years. As Pempel (1978, p. 67) has argued, the consequences of having a dominant party for more than a quarter of a century has been an institutionalization of the relationship between the LDP and the organs of government and between the LDP parliamentarians and the senior ranks of the civil service. Particularly in matters concerning the influence of party in policymaking, satisfaction with the status quo, and the definition of important policy issues, we would expect that this progressive integration of top civil servants with dominant party would produce party and senior bureaucratic elites with similar views, views more similar than between senior and lower level bureaucrats. We have found that this is indeed the case. Thus, the perpetual hegemony of one ruling party also may have contributed to the organizational cleavage within the bureaucracy. With a single party perpetually in power, there is no need for the LDP to penetrate the bureaucracy further than the very top ranks to manage it. There has not been the need for the LDP to "colonize" the bureaucracy to maintain its control in a situation of coalition government and constant cabinet crises, as in Italy. Nor has there been the need to purge bureaucrats whose style and policy perceptions agree too strongly with the rival party previously in power, as has happened in West Germany (Putnam, 1975).

Finally, there is the ideological or political cleavage that also cuts across the politician-bureaucrat institutional cleavage. Here the main lines of divergence ideologically are between the LDP politicians and civil servants on the one side and opposition party politicians on the other, with the latter manifesting deep-rooted alienation from the present political and social arrangements. Even in this case, however, there also is a very complicated pattern because there also exists a consensus on the salience of policy issues including LDP politicians, top bureaucrats, and opposition politicians. Furthermore, LDP and opposition politicians tend to see the political process (e.g., the role of bureaucracy, the influence of the bureaucracy and Diet in policymaking) similarly in contrast to the somewhat different views of bureaucrats.

One of the most interesting implications of our data on views of the salience of policy issues (Table 6) is what it tells us about the current state of partisan polarization. Opposition party politicians do not differ as greatly from LDP politicians and higher bureaucrats in choice of issues, nor does either side attribute as much salience to such issues as defense and education as we might have expected given the polarization of the earlier decades. In general, there is evidence that the extreme polarization of earlier decades has declined, and there may be an emerging consensus among both LDP and opposition politicians and the higher civil servants (and to a lesser extent with the middle-level bureaucrats as well) over the definition of salient policy issues facing Japan.

In part this depolarization can be explained by the decline of the two major parties in ideological conflict, the LDP and the Socialists, and the rise of center parties within the opposition camp. During the 1970s, the LDP obtained less than a majority of the popular vote (and a bare majority of seats), while its chief ideological opponent, the Socialist Party (JSP) also declined from one-third of the votes to approximately one-fifth. Meanwhile, middle-of-the-road opposition parties like the Democratic Socialists and the Clean Government Party, which had not even existed in the mid-1950s, had come to obtain nearly a fifth of the vote by the late 1970s. The Communist Party had also moderated its ideology and converted to a strategy of the "Parliamentary Path to Revolution," thus also contributing to an ideological depolarization in the party system.

The new multipolar power distribution and ideological moderation also led to a new relationship between conservative and opposition politicians in parliament. At the time of our survey (1978), the LDP's seats in parliament had declined to a bare majority. Because of the peculiar norms and procedures of the system of committee voting in the Diet, this meant that for the first time in postwar Japan the opposition parties actually controlled a number of parliamentary committees, and there were signs of an increasing institutionalization of conflict management among the parties in the Diet (see Krauss, in press; Mochizuki, 1982).

Thus, we find several major trends that have developed in parallel to produce institutional, organizational, and political cleavages crosscutting each other in complicated ways. Background, recruitment, and elitist bureaucratic norms derived from a historical tradition of bureaucratic dominance have produced an institutional cleavage between politicians and bureaucrats in their basic views of society, decision making, and the political process. Organizational role differences and the long-term effects of generational change and the institutionalization of a dominant party, however, have led top bureaucrats at least into close collaboration and attitudinal consensus on basic role and policy issues with LDP politicians. Further, changes in parties and the party system and the greater institutionalization of the Diet has helped to mitigate the ideological polarization between government and the opposition, thus bringing opposition politicians also into the consensus with the LDP at least in such areas as political dominance over bureaucracy and the definition of major policy needs.

Our pattern of crosscutting attitudinal cleavages among Japanese political elites helps to explain actual patterns of policy conflicts in Japan. Despite having a homogeneous, elitist, and to some extent antipluralist bureaucracy, Japan has experienced few straight bureaucratic-politician policy conflicts. Rather, as Campbell (in press) has persuasively argued, the most salient conflicts tend to be among "subgovernments," the relatively permanent coalitions of LDP politicians, specific ministries, and interest groups that coalesce around a common issue area and shared vested interests. And, although government-opposition conflict has continued, in recent years it has been less intense and confrontational than in the past.

The Surprising Power of Politicians

Perhaps our most surprising finding is that politicians, parties, and the Diet are perceived by the elites themselves to play a much greater role in policymaking than most models of Japanese politics have described. Instead of a bureaucratic-dominant model of influence in the policymaking process, we have seen that a model of shared and equal influence between bureaucrats and politicians or even of party superiority is more appropriate.

Probably the most important explanation of our results is found in several major factors that have contributed to the increasing influence of politicians in the political process, but which most prior research has underestimated. A brief discussion of these factors, each a counterweight to the arguments for bureaucratic dominance we discussed in our introduction, may make our unexpected results on the power of politicians in policymaking more comprehensible.

First, although the bureaucracy escaped relatively unscathed from the Occupation reforms, it was not the only political institution of the postwar period to emerge strong after the Occupation. Thus, the Prime Minister's office was vastly strengthened under the new political structure; he has the complete power of appointment and removal of all Cabinet ministers without the approval of the Diet. The Diet itself was strengthened too. Unlike in prewar Japan, the parliament has an extensive committee system organized along functional lines and the power of investigation. Thus, the potential for politician's influence to rival bureaucratic influence was established.

More important, the formation of a single, majority ruling party after 1955 vastly strengthened the hand of the professional politician over the career civil servant. Although the bureaucracy may formulate legislation, it is the LDP that decides which bills are to be taken up, which are to be modified and how, and which are to be introduced into the Diet with what priority. There is an extensive and powerful apparatus composed of Diet members within the party structure (especially the Policy Affairs Research Council) which debates and ultimately shapes legislation and legislative priorities. One can argue that although the Diet was merely a ratifier of government policy for much of the earlier postwar period, it was the dominance and pre-Diet control of policy by the LDP and not bureaucratic dominance that caused this situation. Especially in areas of extreme political sensitivity and public interest, such as defense policy, or where the political fallout from key supporting interest groups may result, as in agriculture, the top leaders of the LDP are careful not to leave decisions solely to the bureaucracy (Campbell, in press; Donnelly, in press; Otake, 1981).

Third, the top party leadership has very substantial leverage and influence over the bureaucracy to ensure that its views on policy prevail. No bureaucrat can expect to be elevated to the higher reaches of the civil service (e.g., to bureau chief or administrative vice-minister) without careful scrutiny by the LDP to ensure that he will be politically compatible (Pempel, 1974, p. 653). And the fact that some top bureaucrats have long-range aspirations to run for the Diet as LDP candidates also gives additional leverage to the party leadership. These ambitions ensure that bureaucrats will play ball on crucial matters, lest they risk their post-retirement political futures in the LDP, the only alternative if they wish to join a party in power. Even civil servants who desire to join the top corporate echelons after retirement rather than enter politics must be careful lest a top Party leader ruin the opportunity with a few well-placed words in the business community, a close supporting interest group of the LDP. Ultimately, of course, the overt use of such leverage usually remains unnecessary, since the long-term control over government by the LDP and thirty years of deciding on policy together undoubtedly have made bureaucrats sensitive to the desires and aims of LDP politicians, helping to bring about a relatively smooth working relationship and compatible views on both sides, as our data indicate.

To the power of the LDP must be added the rise of the power of the Diet in the 1970s. As we have indicated, from 1976-1980, LDP seats in both houses of the Diet declined to a bare majority, and the opposition for the first time gained control over some of the committees in the lower house. Because the conservatives needed to compromise with this newfound opposition legislative power, Diet deliberations during this period came to exert a greater influence on the policymaking process than ever before (Krauss, 1982), which is undoubtedly, reflected in our respondents' high evaluations of Diet influence.

Finally, the bureaucracy's near-monopoly control over information and expertise, one of the major sources of its influence in policy, is being challenged of late by politicians. With the long duration of LDP governance and thus with many politicians gaining cabinet level and Diet committee experience, an increasing number of LDP members have developed the skills and knowledge necessary to understand and deal with complex policy issues (Otake, 1981, p. 2). Further, in recent years, LDP Diet members have increasingly formed policy study groups to share information and expertise and to discuss key questions. There has thus been a tendency toward the development of policy "tribes" (*zoku*) within the Party who take a keen interest in, and have great knowledge of, specific policy areas (Otake, 1982; Pempel, 1981, pp. 47-50). Thus, this tendency too buttresses the growing influence of the "pure party politician" vis-à-vis the ex-bureaucrat politician.

To argue that politicians are much more influential in policymaking than they are usually given credit for is not to argue that the bureaucracy is uninfluential. Our data confirm that it is. Further, the bureaucracy undoubtedly was the most influential actor in policymaking in the first decade and probably also the second of the postwar era. It is from this period that the stereotype of bureaucratic dominance emerged and remained unchallenged. We would argue, however, that after approximately 1965, the LDP increased its control over both policy and bureaucracy to the point that it rivals the civil service in its influence. In 1965, for example, the LDP succeeded in intervening in the all-important budget process which until then had been the almost exclusive prerogative of the powerful Ministry of Finance. LDP control over the budget and other crucial policy matters increased subsequently. By 1968 it was even able to implement an "administrative reform" program to inhibit the growth of the national bureaucracy. Unlike other nations where bureaucratic resistance either prevents the adoption of such programs or torpedoes them in practice, the LDP not only put the program into effect, it also succeeded in its aim: Japan today has the smallest national government bureaucracy and the only of all the advanced industrial democracies that has decreased in size over the past fifteen years (Pempel, 1982, pp. 268-269; Rinji Gyōsei Chōsakai Jimukyoku, 1982).

Thus, it took ten years from its formation for the LDP to acquire the information control and expertise necessary to compete with the bureaucracy in policymaking, to gain control over the process of recruitment to top bureaucratic positions, and to force the bureaucracy to realize that it would have to share power with a party likely to be perpetually in power. In the conditions and tradition of bureaucratic dominance in Japan, it seems to have taken a long tenure of single majority-party rule to institutionalize the party's role to the point where it today has at least as great an influence on policymaking as the bureaucracy. During this process, LDP politicians and top bureaucrats have come to share similar perspectives on the process and contents of policy.

The convergence of LDP politicians and top bureaucrats, however, does not mean a confirmation of the ruling triad model either. Our data indicate that, as with the bureaucracy-dominant model, the reality of postwar Japanese politics may be far more complicated than the model suggests. The ruling triad model implies at least three conditions that our data do not confirm. First, it implies something of a unique Japanese elite especially cohesive in social background. We have found that Japanese politicians are not as socially elite as bureaucrats, and that this difference between the two groups is common in most industrialized democracies.

Second, it posits the influence of business in policymaking as equal to LDP politicians and bureaucrats. The latter two elites, however, do not perceive businessmen as their equal in policymaking. To put it another way, if business, bureaucracy, and the LDP are the three legs of a tripod supporting the Japanese state, then one leg is shorter than the other two. Other studies also confirm that the influence of big business on policymaking, especially on issues outside the economic ones of direct concern to them, may be exaggerated, and that when business does exercise influence, it is hardly as a monolithic bloc (Curtis, 1975; Otake, 1979).

Along similar lines, the ruling triad model also suggests that both the whole and the parts of the trio are cohesive and well integrated and that policy is made exclusively by informal consensus among these actors. Not only may business not be cohesive, but our survey finds important divisions in the bureaucracy between upper- and middle-level ranks in their perceptions of roles and policy. Inter-ministry conflict, which is often severe in Japan, would provide another important line of cleavage within the bureaucracy (Campbell, in press; Craig, 1975, pp. 15-17). And, our survey shows that decision making may not be limited to the behind-the-scenes consensus of LDP, bureaucracy, and big business; the Diet process, including the participation of the opposition parties, also may play some role in policy-making. (See also Mochizuki, 1982; Otake et al., 1982.)

Japanese Politicians and Bureaucrats in Comparative Perspective

We have found Japan to share in the trend toward partial convergence of politicians and bureaucrats in policymaking among the advanced industrialized democracies. Both elites have recognized roles and influence in the policymaking process, although there are still some differences in those roles and a rather large difference in the styles in which they approach and perform those roles. In terms of those differences, Japan is closer to Italy in having a highly elite, homogeneous bureaucracy with strong classical bureaucratic attitudes toward their role and some negative attitudes toward a pluralist political process. These aspects of Japanese civil service political culture undoubtedly spring from bureaucracy's powerful role in Japan's late modernization in the nineteenth century and in Japan's recent experience with prewar authoritarian rule.

Yet what might be a troublesome institutional cleavage or paralysis of policymaking owing to these differences is, in the case of Japan, mitigated by a consensus especially between top bureaucrats and LDP politicians (a consensus in which opposition politicians increasingly are participating) about the role bureaucracy plays, the influence of political agencies in policymaking, and definitions of significant policy issues. One of the most important factors making for this cross-cutting consensus, we have argued, has been the long-term majority dominance of the LDP as a ruling party. The extent to which latent conflicts between bureaucrats and politicians could remain in the background should the LDP be forced to alternate or share power with another political party, as all the other governing parties in all advanced industrial democracies have had to do, must remain an open question at this time.

The Liberal Democratic Party tenure as a long-term dominant party has also contributed to the increasing influence of politicians in policymaking, a surprising finding given the usual bureaucracy-dominant model of Japanese policymaking. There is nothing contradictory nor unique about a modern democratic policymaking process in which *both* politicians and bureaucrats exercise great influence, or in which a majority party steadily increases its power even in a system with an elitist and powerful bureaucracy. After discussing the influence and elitism of the French bureaucracy, Suleiman (1974, pp. 352-371) nevertheless concludes that under the Fifth Republic the emergence of a majority Gaullist party led to greater political control over the bureaucracy and over policy. We would argue, on the basis of our data and the factors discussed above, a similar process has been occurring in Japan.

Finally, the Japanese variant of the universal trend toward partial convergence of politicians and bureaucrats in the advanced industrialized democracies leads to a hypothesis about Weber's prediction that the inheritors of the modern state would be the professional politician and the professional bureaucrat. We have confirmed that this trend exists in Japan as well as in Europe and the United States. The timing and pace of that ascent to power of each elite may vary, however. In the Anglo-American democracies especially, it is the bureaucracy's role that in the mid-twentieth century has appeared to expand most rapidly. This expansion of bureaucratic power, resulting from the ever-growing complexities of the modern state and the ever-increasing number and legitimacy of organized interst group demands, thus has come to impinge on the established dominance of politicians by giving the bureaucracy a role in the formulation of policy and the management of interest group conflict.

Elsewhere in modern democratic states a similar phenomenon has occurred. But in nations such as Japan, and further study would probably demonstrate in such states as Italy, Germany, and probably France, that expansion is built on an already solid base of bureaucratic influence. In those nations, where the bureaucracy played a major role both in modernization and in an authoritarian interlude, and full-fledged democracy was a post-World War II phenomenon, the process of partial convergence was reversed. Bureaucratic power and policymaking influence always existed, and bureaucrats have always been more than mere implementors of policies decided upon by politicians. Rather, the two elites partially resemble each other today and share in influence in the political process primarily because *politicians* in the postwar era have rapidly come to take on the major roles in policymaking that they have always had in the Anglo-American democracies. In other words, the case of Japan suggests that politician-bureaucrat roles in policymaking have converged to a roughly similar pattern in all advanced industrial democracies, but there have been not one, but two roads to that contemporary endproduct: a more rapid expansion of the bureaucratic role to converge with a previously dominant political elite and a more rapid expansion of the politicians' role to converge with a previously dominant bureaucratic elite. All roads may lead to Rome, or Tokyo, Paris, London, and Washington, but those roads may have been paved at different times and in different ways.

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Do Cooperators Exit More Readily than Defectors?

JOHN M. ORBELL
University of Oregon

PEREGRINE SCHWARTZ-SHEA
State University of New York College at Cortland

RANDY T. SIMMONS
Utah State University

Standard prisoners' dilemma games offer players the binary choice between cooperating and defecting, but in a related game there is the third possibility of leaving the game altogether. We conceptualize exiting as taking the individual beyond the reach of externalities generated in the original group, and on that basis—together with the assumption of self-interested (dollar-maximizing) behavior on the part of all players—we derive the prediction that the exit option will drain the community or group more of cooperators than of defectors.

But experimental data do not support this prediction; cooperators do not leave more frequently than defectors and, in fact, there is evidence that defectors are more prone to leave than cooperators. We consider and reject the possibility that this failure of prediction results from the (admitted) greater optimism of cooperators about the incidence of cooperation "here," and present data supporting the hypothesis that cooperators often stay when their personal interest is with exiting because of the same ethical or group-regarding impulse that (presumably) led them to cooperate in the first place. Cooperation can be produced for a group or community either by inducing people to cooperate or by inducing those who are going to cooperate to stay in the game, and ethical considerations seem to underlie the decision to stay as well as the decision to cooperate while staying.

In prisoners' dilemma games, as in the real world situations that they model, cooperators can expect lower payoffs than defectors. This is a clear consequence of the dominance of the defect alternative; no matter what others do, defect always pays more than cooperate. But of course it is cooperators who, by their personally costly action, produce the collective gain. Thus, what promotes cooperation becomes an important question. Reasonably enough, it has been the focus of much theoretical and experimental work.

The rate of cooperation can be influenced in two ways: by persuading people who are locked into the game to cooperate, *and by persuading people who are going to cooperate to stay in the game when they have the option of leaving.* This article is an experimental investigation of what happens when players in an *n*-prisoners' dilemma game are offered a possibly attractive exit option. There are *a priori* reasons associated with the lower cooperator's payoff "here" to predict that cooperators will be more attracted to this option than defectors, but in our experiments they do not take it any more than defectors; to the contrary, defectors appear somewhat more prone to leave than cooperators. In what follows, we first derive our theoretical expectations and then describe our experimental test. Granted our predictions are not met, the interesting question becomes: Why? We will present data relevant to several possible explanations. Our conclusion, to anticipate, is that the exit-stay decision—like the cooperate-defect one—is underlain by normative considerations that produce behavior contrary to the assumption that subjects are maximizing their "game" or "dollar" self-interest.

In its standard binary choice definition, the prisoners' dilemma offers no escape from the externalities that are the crux of the problem. The game assumes a fixed population, and its unfortunate players have no alternative but to stay and accept the payoff that is dictated by their personal cooperate-defect choice and the concurrent

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cooperate-defect choices of others.[1] By suggesting a game in which there is an exit opportunity, we are suggesting a trinary choice game that is no longer classically prisoners' dilemma. But the usefulness of formulations such as the prisoners' dilemma consists in their capacity to capture the essentials of naturally occurring situations of theoretical interest, and, we suggest, there are many such situations which players have, in addition to the standard prisoners' dilemma choice between cooperation and defection, a third exiting opportunity. Consider the following illustrations.

1. Members of an organization (such as a university) must choose between investing time, energy, and money in actions to improve the quality of the organization (generally "voice") and not doing so. Suppose that the invest decision is more costly to the individual regardless of what others do, but that the costs to the individual from investing are less than the benefit to the organization from each invest decision. The organization will be in better shape if everyone invests than if nobody does, but there is a dominant incentive coupled with a suboptimal equilibrium, the defining characteristics of a prisoners' dilemma as well as several related games (Dawes, 1975). *In addition to investing and not investing, however, individuals can often move to a different organization.*

2. Downtown businesspeople often must decide between contributing money toward amenities designed to improve the business climate of the downtown area (parking buildings, a mall, and other conveniences) and not contributing. Suppose a situation in which all will be substantially better off if all contribute than if nobody does (total benefit from a contribution exceeds total cost), but in which not contributing is dominant. The game is again prisoners' dilemma, and the *a priori* prediction is that nobody will contribute. *But businesspeople confronted with such dilemmas can move to attractive suburban locations quite independent of the downtown area.*

3. Workers in an industry can be faced with the choice between pulling together in union activities and not doing so. In the classic formulation (Olson, 1965), all will be better off if all do so than if none do so, but individual workers will be better off personally by free riding on the successful action of others. *Except in highly controlled economies, however, they do not have to stay in the industry or unit in which the organizing effort is being made.*

4. Members of a military alliance can choose to contribute a large part of their gross national product (GNP) toward the common defense, or they can choose to free ride on the contributions of others by contributing relatively little. Suppose a situation in which the security of all will be assured if all contribute at some given level of GNP, but in which that security will decline as the level of contributions declines. As Olson and Zeckhauser (1966) have argued, there will be an incentive for nations to free ride by under-contributing—particularly for the smaller ones whose absolute contributions will not be critical, and who can rely on the larger ones to contribute at or above the required level. *But nations can also leave the alliance and attend to their security on their own.*

Notice that an exit option is not necessarily available in games that are prisoners' dilemma or, more accurately, that exit might be prohibitively costly or unavailable in technological terms. Members of a cartel such as OPEC cannot escape the price effects of a decision to break with the cartel policies restricting production or maintaining price (although, no doubt, they could decide to go into some other line of business). And nations locked in an arms race might decide to disarm unilaterally rather than continue to compete, but they cannot stop the world and escape. The binary choice prisoners' dilemma clearly is, therefore, a reasonable representation of at least some naturally occurring situations, and the appropriateness of the trinary model to the particular situation must be decided case by case. Our point is simply that many natural situations involve the trinary choice (cooperation, defection, or exiting) and should be modeled as such.

Are certain kinds of players more prone than others to take the exit option? Is there any difference between cooperators and defectors in this respect? The answer is of some consequence to the successful resolution of such dilemmas. If defectors are more prone to leave than cooperators, then the proportion of cooperators remaining in the group will be higher than without a viable exit option, and the success of the group (albeit now a smaller group) will be greater than without such an option. Conversely, if cooperators are more prone to leave, the availability of the exit option will have made things worse. Short of some mechanism that prevents one population or the other from leaving, defectors and cooperators are just as capable of taking the exit option, and the issue becomes an empirical one: Will defectors or cooperators select themselves out disproportionately in the presence of an exit option?

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[1] Equally, the absence of an escape opportunity is part of the standard definition of public goods. This fact is normally clouded somewhat by the ambiguity of the word "goods," but, as Barry and Hardin (1982, p. 184) point out, the criterion "impossibility of exclusion" implies the criterion "impossibility of rejection" in the case of public bads. A public product is a product from which one cannot (at reasonable expense) be excluded and from which one cannot (at reasonable expense) escape—as well, of course, as being one involving "jointness of supply."

The prisoners' dilemma is of fundamental importance to political theory, whether we are concerned with the justification for collective action and the state itself (Buchanan, 1975, for example), the logic of collective action (Hardin, 1982; Olson, 1965), the structure of normative systems (Campbell, 1975; Ullmann-Margalit, 1977), or the preservation of natural resources (Hardin, 1968; Hardin & Baden, 1977). We propose that much is to be gained by thinking about the consequences of adding an exit option to the standard binary choice prisoners' dilemma. In 1956 Tiebout pointed out the significance of "voting with your feet" to the provision of public goods in metropolitan areas, and in 1970 Hirschman alerted us to the general importance of the exit option to what happens in a community or group. What, then, of prisoners' dilemma games in which an exit alternative is possible?

Reasons for Expecting Cooperators to Leave More Readily

In the standard binary *n*-prisoners' dilemma, payoffs to cooperators and defectors alike are a function of the number of people cooperating. Specifically, the addition of marginal cooperators (or the deletion of marginal defectors in the "bads" version of the game) adds a constant increment, more than the incentive to defect, to the payoffs of all.[2] All players in this standard (symmetrical) game consume whatever externalities are produced so that, in a real sense, "group member" is defined by this consumption of externalities. Thinking in these terms has led Head (1973) to suggest that appropriate boundaries for political units might be defined by the "reach" of externalities and, indeed, we might attempt a definition of the elusive notion of community in a similar way.[3]

By the same token, the idea of externalities lets us define a person who exits as one who, in some way, moves beyond the reach of whatever external effects are generated by group members. This might be accomplished, of course, by various tactics short of geographic movement (one can sometimes withdraw from a group while remaining in the same location), but geographic movement is the classic form of exiting, and we will talk about it in those terms. Thus, in the context of the prisoners' dilemma, a person who exits is one whose payoff is not dependent on the number of group members cooperating and defecting. The exiting person is freed from group dependency, either beneficial or harmful.[4]

Let us now make three important assumptions.

1. *Exit payoffs available to cooperators and defectors are the same or are believed by them to be the same.* It might be, of course, that cooperators—persons who have already accepted a lower payoff here—expect, and should expect, a lower payoff there. Having taken group-regarding action in one place, cooperators might well anticipate doing the same in another, and anticipate correspondingly lower payoffs there than the defectors. But this is somewhat ad hoc and, assuming equal exit opportunities is at least a good place to start.

2. *Cooperators and defectors have the same expectations about the number here who will cooperate.* In the standard prisoners' dilemma game, where the defect alternative is unambiguously dominant, expectations about what other group members will do are irrelevant to the individual's choice; whatever others are expected to do, the defect alternative always pays more. But, as we will see, adding the exit option to the stan-

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[2] A necessary characteristic of the prisoners' dilemma is that the social cost of defection is greater than the private benefit going to the defector. If it were not, of course, net cost would be less than net benefit, and the action would be socially efficient; in the prisoners' dilemma, social inefficiency results from the individual's (privately efficient) choices.

[3] The problem gets more complicated, of course, when we recognize that there will often be a decay effect in externalities as one moves further from their source. In the present article we overlook such niceties and develop our model simply in terms of a step function; to do anything else would have complicated our design problems far beyond their already considerable difficulty. Notice, however, that the problem of such decay effects has a parallel in the problem of imperfect public goods. As we pointed out in note 1, the criterion "impossibility of rejection" is at least implied in the definition of public goods. But many authors (Goldin, 1977, for example) have argued persuasively that examples of pure public goods are very hard to find and that the externalities involved in most alleged cases are consumed unequally by individuals constituting the group in question.

[4] The exit alternative might sometimes plausibly be conceptualized as the defect one (or perhaps even the cooperate one), but it is important to remember that externalities are the central fact of the prisoners' dilemma and that the standard game has all players both consuming and generating such externalities. If exiting is to be so conceptualized, then—to keep the game prisoners' dilemma—exiting players would have to continue such consumption and generation of externalities. This leads to messy argumentation, and it seems far cleaner simply to acknowledge that there is often the alternative of leaving the game altogether and to build that possibility into the analysis.

dard binary choice can make the behavior of others here highly relevant to the choices of dollar or game utility-maximizing individuals and, like the first, this assumption is a useful one for deriving initial predictions.

3. *When it comes to the choice between exiting and staying, all players are motivated in the same dollar or game utility-maximizing manner.* Although we have defined an exiting person as one who moves beyond the reach of external effects generated within the sphere, it is apparent that the act of exiting itself does have consequences for group members. If a person who would otherwise have cooperated leaves, the group is deprived of the benefit his or her presence would have involved. And, conversely, if a person who would otherwise have defected leaves, the group is spared the cost his or her presence would have involved. We term such effects "second order" externalities. Their presence might well trouble an ethically inclined person, and it is possible that cooperators—people who have (presumably) been constrained from defecting by the presence of first order externalities—might be constrained from exit by the fact of their presence. But before we examine that possibility we will derive and test predictions from the more simple assumption that external effects are not a consideration for players making the exit-stay choice.

The predictions follow readily. Cooperators stand to get less here than defectors in the prisoners' dilemma (by definition) and therefore will find the exit alternative (constant for cooperators and defectors by assumption 1) relatively more attractive. Alternatively—stated in terms of what happens here—cooperators will need to expect more group members to cooperate (something precluded by assumption 2) in order for their cooperate payoff to exceed the exit payoff than will defectors in order for their defect payoff to exceed that same exit payoff.

The graphics in Figure 1 should make the point clear. Consistent with Schelling (1973) and others, the number cooperating here is measured from left to right on the horizontal. The functions DEFECT and COOPERATE indicate, respectively, game utility or dollar payoffs to those choosing to defect or cooperate; these have positive slopes because of the constant increment going to all from successive cooperating decisions. The function EXIT, constant with respect to the number cooperating, indicates payoffs to those exiting. Clearly, in order for cooperators to see staying (and accepting the cooperate payoff) as more attractive than exiting, they will have to expect y group members to cooperate, a number substantially more than the equivalent number, x, for defectors. There will be, of course, some exit functions by which staying will be more attractive for both cooperators and defectors, just as there will be some by which staying will be less attractive for both. *But where that is not the case, and within the specified assumptions, cooperators will leave more readily than defectors.*

Experimental Design

Groups of nine subjects, recruited by advertisements in the local daily and student newspapers, were offered the choice between O (cooperate) and X (defect) in the context of the payoff matrix in Table 1.[5] (It will be noted that this is a standard

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[5] The number nine was a compromise between logistics and theoretical interest. Because our ultimate interest (in common, no doubt, with the interest of most *n*-prisoners' dilemma researchers) is with naturally occurring groups that are large, and because there is considerable theory relating behavior in prisoners' dilemma games to the size of groups, we would have liked to deal with groups of substantially larger size. But logistical constraints meant that groups of nine subjects were about as large as we could handle. The decision was supported by financial considerations: running the *number* of groups that was necessary for confidence in our results would have been prohibitively expensive, had we been able to overcome the logistical problems associated with groups larger than nine.

Subjects were signed up for particular experimental sessions on the basis of their convenience and the availability of positions. Care was taken to prevent groups of friends from participating in the same sessions—although, of course, there is something to be said for the security of having friends around in such psychology experiments and subjects might have lied to us. Similarly, we screened subjects as carefully as we were able in order to prevent their signing up in more than one session for this particular sequence. About half of our sessions were run in Logan, Utah, and half in Eugene, Oregon. We cannot detect any difference in the behavior of subjects drawn from these two subject pools in terms of the incidence of cooperation and defection, or the incidence of exiting. Although we did not keep any formal record of such things, our impression is that perhaps one-third of all subjects were non-students in Eugene and somewhat less than that in Logan. Because of our strict anonymity requirements, we have no way of knowing whether behavior differed between students and non-students, or indeed among students of different kinds. (Marwell & Ames, 1980, provide evidence that students with certain majors—notably economics majors—are more prone to free ride than others.) We point out, however, that our interest is less in the absoute incidence of cooperation and defection, or of staying and leaving, than it is in the relative incidence of such things between our various experimental conditions. Although there are plenty of intuitive reasons for hypothesizing different marginal rates of cooperation in different groups (different patterns of student socialization would seem to make Marwell and

Ames's findings not too surprising), reasons for hypothesizing that different subject populations will respond differently to our particular experimental variables are less apparent.

On arrival, subjects read and signed an informed-consent form promising them, among other things, that their choices would be strictly anonymous. Upon conclusion of the experiment, subjects were released one by one from the experiment room and sent to a "payoff room" where they were told the result and paid accordingly. Prior subjects were in the elevator and well clear of the area before subsequent ones were sent to the payoff room.

n-prisoners' dilemma matrix, with the defect payoff dominating the cooperate one and the all-cooperate payoff superior to the all-defect one; defectors get a constant $5.00 more than cooperators.) Once their decisions had been recorded, subjects were told that they had a further choice to make—between E and Not E. If they chose E (to exit), their payoff would "not be dependent on the choices of any others in the group." Instead, they would have an equal chance of making any one of the payoffs in the original O/X Matrix (Table 1); they would select (blind) one poker chip with a dollar value on it from among

Figure 1

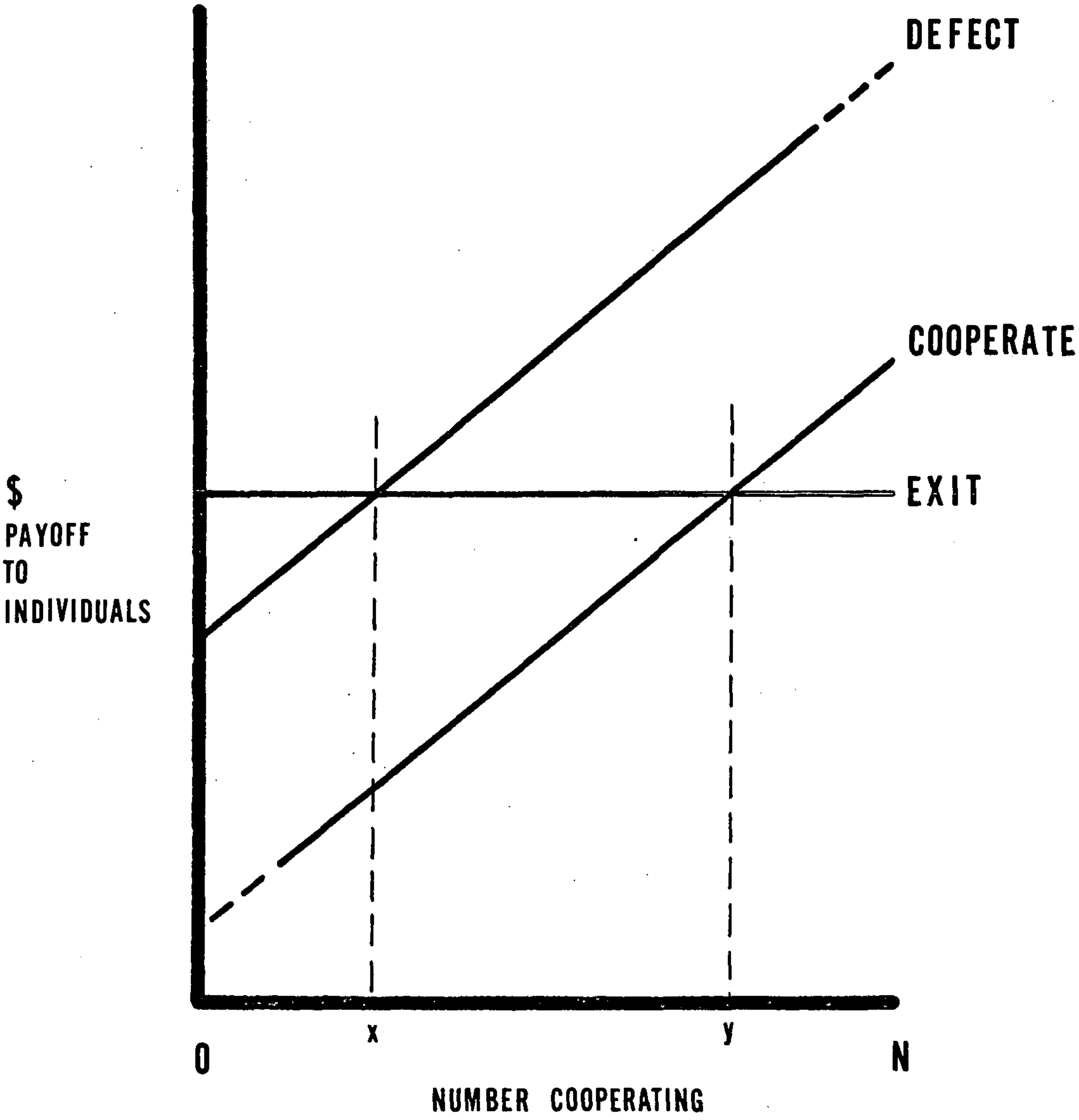

Table 1. Nine-Person Payoff Matrix

Number Choosing 0	Number Choosing X	Payoffs to Those Choosing 0	Payoffs to Those Choosing X
0	9	–	$ 5.00
1	8	$ 1.00	6.00
2	7	2.25	7.25
3	6	3.25	8.25
4	5	4.50	9.50
5	4	5.50	10.50
6	3	6.75	11.75
7	2	7.75	12.75
8	1	9.00	14.00
9	0	10.00	

eighteen in a paper bag. If they chose Not E (to stay), their payoff would be determined by their original choice between O and X, taking account of others' choices.

We decided to use this lottery device for those who chose the E alternative in recognition of the fact that most real-world exit alternatives would involve at least a measure of uncertainty. Although people who exit will normally have a rough idea of what to expect in a new location, they will rarely have certain knowledge. Knowing that all the O/X payoffs were included with an equal probability would make the expected value of $7.50 easy to recognize.

Deciding what payoffs to offer those who did *not* choose E—those who stayed—proved somewhat more complex. A rational player trying to decide between E and Not E would have an easy enough time figuring the expected value of leaving, but the expected value of staying, granted the prior choice between O and X, would have to take account of the fact that some players would be choosing E and that some of those would be O choosers—with the number of *staying* O choosers determining payoffs for staying cooperators and defectors. At this point, we came to recognize that any change in the number of players in an *n*-prisoners' dilemma game *would inevitably change some of the critical parameters of the game* and, therefore, would change the incentive structure for players in the game. Accordingly, we had to make some decisions about what parameters would be held constant and were obliged to present subjects with a set of matrices, each specifying the consequences of an O and an X choice for a different number of players staying in the game—and with the set embodying our decision about which parameters were most important.[6] The matrices we used are given in Appendix 1. It will be seen that we have kept the all-choose-O payoff constant at $10.00; the all-choose-X payoff constant at $5.00; and that X choosers always make a constant $5.00 more than cooperators. A cost of these decisions, however, was that the incentive to X (the difference between an O payoff for the *n*th cooperator and the X payoff for the outcome with one less cooperator) got progressively less with the smaller numbers involved so that, by the case of the two-person matrix, the game was not prisoners' dilemma. A further cost was that the payoffs for lone X choosers and lone O choosers (lone free riders and lone suckers, respectively) varied between matrices. Some such costs were inevitable, however, and in our judgment these were less likely to bias the exiting rates of cooperators and defectors than the alternatives.

Although the logic behind this set of matrices is relatively complex, our subjects appeared to have little trouble recognizing how to use the set when they were making their choices between E and Not-E. We were, of course, at pains to make sure that subjects understood what was involved and asked repeated test questions to convince ourselves of that before continuing.[7]

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[6] This problem in the reduction (or increase) of group size has the important consequence that one cannot analyze the effects of a change in numbers by itself; with any change in numbers, some other parameters must change with many of the parameters in question being relevant to the incentive structure of the game. A considerable literature is concerned with the consequences of varying *n* in prisoners' dilemmas and related games, but variation of *n* cannot be isolated from other parametric changes. Accordingly, theorizing about what happens in groups of different sizes *must* take account of necessary parametric changes before conclusions are drawn about the consequences of size per se. Or, alternatively put, although effects of *n* per se are certainly imaginable, those effects should not be confused with the effects of parametric changes associated with changes in *n*. The logic of this problem, together with a specification of the parametric trade-offs that are involved, is spelled out in van de Kragt (1983). Bonacich et al. (1976) recognize the problem.

[7] Instructions read to subjects are available, on request, from any of the authors.

Within these broad terms, we crossed a no-discussion/discussion variable with a variation in the magnitude of the exit payoff. In the no-discussion condition, no verbal communication of any kind was permitted (we could not, of course, prevent nonverbal communication and other messages from flowing among our subjects), but in the discussion condition we gave subjects ten minutes of free discussion before making the choice between O and X.[8] A standard finding of experimental research into behavior in *n*-prisoners' dilemmas is that a period of group discussion substantially increases the incidence of cooperation (Dawes, McTavish & Shaklee, 1977, for example), and it seemed quite plausible that discussion could also influence exiting rates, perhaps differently for cooperators and defectors.

The low exit incentive involved the lottery among the payoffs specified in Table 1 with the expected value of $7.50. The exit function portrayed in Figure 1 is this low value; the expected value of exiting here is superior to the defect or X payoff when fewer than 25% cooperate and is superior to the cooperate or O payoff when fewer than 75% cooperate. A high exit incentive was offered by a similar lottery, but one in which $3.00 was added to all these low values. For this condition, therefore, the expected value of exiting was $10.50, superior to the defect payoff when fewer than 50% cooperated, but superior to all cooperate payoffs that were available; cooperators could expect to make more by leaving than they could by staying, regardless of what happened here.

This variation in the attractiveness of the exit alternative was introduced in order to allow for the possibility that behavior would differ at different incentive levels. In particular, we were interested in how cooperators would respond when what they could make here was less than what they could reasonably expect there and when, therefore, not even the most optimistic expectations about what would happen here could justify a stay decision in dollar terms. We therefore ran the following four conditions:

Low exit payoff, discussion (seven groups)
Low exit payoff, no discussion (seven groups)
High exit payoff, discussion (eight groups)
High exit payoff, no discussion (seven groups)[9]

Simultaneous Choice Conditions

Because of our need to know about the X v. O choices that would have been made by subjects choosing E (had they stayed), we were obliged, in our basic design, to offer the cooperate v. defect choice first and then the exit v. stay decision subsequently, but real-world decision making is often more messy than this, with players deciding between staying and leaving and between cooperating and defecting at the same time. Accordingly, we ran a few discussion and no-discussion sessions with the lower expected value for exiting and with subjects making a simultaneous choice between O, X and E.

Table 2 presents our results for all the sequential choice conditions. As will be seen, in the low exit incentive-discussion condition only a single subject exited—one who had previously chosen to defect—and 49/62 (79%) of those remaining cooperated. By comparison, in the equivalent version of the simultaneous choice condition nobody exited and 15/18 (83%) cooperated.[10] In the sequential choice, low exit incentive condition without discussion, 16/63 (25%) exited, with 11/47 (23%) of those remaining cooperating. By comparison, in the equivalent version of the simultaneous choice condition 11/45 (24%) chose to exit with 6/34 (18%) of those staying choosing to cooperate. The differences are not significant, and we can proceed without undue concern that our sequencing of the cooperate-defect choice before the stay-exit choice altered our findings substantially.[11]

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[8] Permitting discussion in one condition and not in the other meant that other things, concomitant with the discussion period, also varied—most notably, the time between being instructed about the choice alternatives and actual decision making. Such "thinking time" might, plausibly enough, influence the outcome of a normatively loaded decision such as the one between O and X. To the extent that cooperation is produced by normative constraint (Campbell, 1975), for example, thinking time could serve to alert subjects to the normative implications of the alternative choices. But Dawes and Orbell (1982) show that thinking time per se has no apparent effect on decision making, so we can comfortably ignore this difference between the two conditions.

[9] Our objective was to run eight groups n each of these four conditions, but in three cases insufficient subjects kept their appointments (even allowing for the two extras we normally signed up) to make it possible to proceed.

[10] We terminated the simultaneous choice-and-discussion series at two sessions only when it became apparent that staying would be essentially universal and that cooperation rates would not differ significantly from those in the simultaneous choice condition. We conducted five simultaneous choice-and-no-discussion sessions.

Is the Exit Option More Attractive to Cooperators?

From the data in Table 2 it appears that it is not; if anything, it appears that it is more attractive to defectors, in direct contrast to our prediction. In the low-exit-incentive-with-discussion condition the absence of any exiting cooperators and the presence of only one exiting defector precludes test of significance. In the low-exit-incentive-without-discussion and the high-exit-incentive-with-discussion conditions there is no significant association between cooperation v. defection and exiting v. staying, but in the high-exit-incentive-with-discussion condition, there is a significant association *with exiting more frequent among defectors.*

This greater propensity of defectors to exit in one of the four conditions—coupled with some suggestion of that (albeit not significant) in two others—is provocative. Collapsing across the four subtables does produce a significant association (chi square = 16.31; p = .0002), but is a procedure subject to Simpson's paradox (simple base rate differences without contingencies may, when collapsed, indicate a contingency). The four phi values that can be calculated on the subtables can be converted to Fisher Z score since a phi is simply a Pearson product moment correlation between 0 and 1. A sufficient condition for testing the null hypothesis concerning linear combinations of the Z scores is that the variables themselves be normally distributed, but clearly the variables, consisting of 0s and 1s, are not. Nevertheless, to test the null hypothesis that a particular Z score is derived from a population where rho = 0, the normal distribution assumption is not necessary. Here, in lieu of a better test, we compute the critical ratio—the average Fisher Z score divided by the square root of the sum of the variances, each equal to $1/16\ (n-3)$ where n is the sample size of the subtable. The value is 2.33—again compatible with the existence of an effect in the direction opposite to that hypothesized. Neither of these manipulations is fully satisfactory, however, and the more conservative interpretation of the data is that the general propensity of defectors to leave more readily than cooperators is not proved.

Notice three other things from Table 2. First, the period of group discussion is associated with a drastic increase in cooperation in both the high and low exit conditions.[12] This potency of group

Table 2. The Frequency of Cooperation and Defection by Exit or Stay Decision, Sequential Choice Conditions

	Low Exit Incentive				High Exit Incentive			
	Discussion		No Discussion		Discussion		No Discussion	
	Cooperate	Defect	Cooperate	Defect	Cooperate	Defect	Cooperate	Defect
Exit	0	1	3	13	14	3	4	25
Stay	49	13	11	36	48	7	12	22
	a		Chi Square = .150		Chi Square = .263		Chi Square = 3.819	
			p = .70		p = .61		p = .05	
							Corrected Chi Square[b] = 2.768	
							p = .10	

[a] Because of the very low expected cell frequency in the exit row, the chi square computation is not possible.

[b] All statistics are computed with SPSS. The corrected chi square is a correction for violation of the continuity assumption of the chi square-distribution in 2 × 2 tables. Loether and McTavish (1976) suggest making this correction for 2 × 2 tables when "the computed [chi square] is in the critical region but near the boundary of that critical region. . ." (p. 549). The correction formula can be found in Norusis (1979, p. 13).

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[11] The low incidence of exiting in the discussion conditions makes tests of significance there inappropriate. Comparing the incidence of exiting in the two versions of the no discussion condition: chi square = .013, p = .91. Comparing the incidence of cooperation and defection after exit in these two versions: chi square = .394, p = .53.

[12] In the low exit-incentive condition before exit, chi square = 38.89, p = .000; in the low exit incentive condition after exit, chi square = 33.43, p = .000. In the

discussion is a standard observation in experimental research on dilemma behavior (e.g., Dawes, McTavish, & Shaklee, 1977; Edney & Harper, 1978; Jerdee & Rosen, 1974; Rapoport, T., 1976, for example) and not directly relevant to the point being made here, but we will return to it later.

Second, notice that the number of players taking the exit option is significantly higher with the higher exit incentive, which is comforting to the idea that players are responding in a systematic manner to the game incentives offered them.[13] Finally, notice that the incidence of exiting is significantly lower under discussion than under no discussion.[14] This latter point we will also return to shortly.

But why are our initial predictions in error?

Why Do the Cooperators Stay?

It might be argued that the results come from a failure of our particular experimental method rather than from a failure of our theorizing. Thus, if our subjects were indifferent to the payoffs being offered them—if those payoffs were trivial or small relative to the wealth or income of the subjects—then, perhaps, they would be prepared to ignore the niceties of costs and benefits associated with the exit option, and simply toss a coin in deciding whether to stay or to leave. If they did that, the argument would go, we could expect no difference between cooperators and defectors in exiting rates, assuming their coins were equally weighted.

We do not accept this argument. It depends on the assertion that our payoff incentives are not taken seriously by subjects—at least not seriously enough to merit a more calculated response—and this assertion runs quite contrary to our impressions (albeit informal) of the experiments. High levels of involvement and affect were characteristically expressed during the group discussion periods and to the experimenters in the payoff room after the results were made known to the subjects. (Perhaps this was faked, but we see no reason for faking here.) Comparable impressions, together with anecdotal evidence, are reported by Dawes et al. (1977). Short of evidence that the dollar differences involved were not meaningful to our subjects, we feel that the explanation for our finding must be sought in the shortcomings of the model from which we derived our initial predictions.

In naturally occurring situations, it is possible that cooperators do anticipate lower payoffs there than do defectors (projecting from their anticipation here, for example). Such pessimism about farther fields, contrary to assumption 1, might then serve to compensate for pessimism about the likely payoffs here from which our predictions are derived, and might be sufficient to keep the cooperators here in greater numbers than would otherwise be the case. Such a possibility cannot be verified without appropriate field research, but it is not particularly relevant to explaining the present experimental results. Here cooperators and defectors have been presented with the same exit incentive and—unless cooperators are more pessimistic about the outcome of the lottery than the defectors (and we see no reason for that being the case)—the formal prediction still stands. How can we account for the fact that cooperators do not leave more readily when their anticipated payoff there is the same as for defectors?

Re-examining assumption 2 we can propose: *Cooperators do not leave more readily than defectors because (contrary to assumption 2), they are more optimistic than defectors about the number here who will cooperate, such optimism compensating for the fact that they can expect less here than defectors with constant expectations.*

The general argument can be seen readily from the low exit parameters portrayed in Figure 1. As dollar maximizers, defectors who expect more than 25% cooperation should stay (the exit function is below the defect one to the right of *x*), and those who expect less cooperation than that should leave, whereas cooperators who expect more than 75% cooperation should stay (the exit function is below the cooperate one to the right of *y*), and those who expect less cooperation than that should leave. (Cooperators and defectors who expect exactly 25% and 75%, respectively, will, presumably, toss a coin.) But if the distribution of cooperators' expectations were substantially higher than that of defectors—if they expected substantially more cooperation here than defectors—then the basis for the prediction could vanish; in fact, some levels of optimism could leave them staying more than defectors. The hypothesis is provocative if for no other reason than the implication that it is cooperators' optimism about their fellows' behavior that is

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high exit incentive condition before exit, chi square = 50.77, p = .000; in the high exit incentive condition after exit, chi square = 25.84, p = .000.

[13] Appropriately collapsing the information in Table 2 shows that in the low exit-incentive conditions 17 subjects (13.5%) of a total of 126 exited, whereas in the high exit-incentive conditions 46 subjects (34.1%) out of 135 chose to exit. Chi square is 15.08 with 1 *df*, p = .0001.

[14] Rewriting the information in Table 2 shows that in the discussion conditions 18 subjects (13.3%) of a total of 135 exited, whereas in the no-discussion conditions 45 subjects (35.7%) out of 126 chose to exit. Chi square is 17.83 with 1 df, p = .0000.

Table 3. Mean Expectations about Cooperation, by Condition and Behavior

	Low exit incentive		High exit incentive	
	Discussion	No discussion	Discussion	No discussion
Cooperators	8.22	6.69	8.59	6.91
n	49	13	61	11
Defectors	6.46	3.13	5.60	3.07
n	13	48	10	41
$T =$	5.76	5.99	9.80	6.86
$df =$	60	59	69	50
$p =$	.000	.000	.000	.000

keeping them from leaving—a classic self-fulfilling prophecy.

Consistent with Dawes et al. (1977) and others, our data show that cooperators are more optimistic about their fellows' behavior than defectors. Table 3 gives the mean number of cooperators expected in the four basic conditions by the subject's own behavior. (Notice that the cooperators' expectations of their own behavior are included in these data. The model has it that players make their stay-v.-leave decisions on the basis of the total expected incidence of cooperation in the group, so their own intended behavior should be included as a contribution to that total.) Although both cooperators and defectors are more optimistic in the discussion conditions than in the no discussion ones, cooperators are consistently and significantly more optimistic than defectors, and it is worth noting, they are generally more unrealistic than defectors, too.[15] We can compare these data with the fact that an average of 7.00, 2.00, 7.75, and 2.29 players actually did cooperate in the respective conditions and see that in only one (the third) were the cooperators closer to the mark than defectors. If the hypothesis is correct, then we might add the further implication that it is cooperators' unrealistic optimism about their fellows that is maintaining the proportion of cooperators in the group.

Unfortunately, the small number of exiting subjects in the low exit incentive conditions means that holding expectations constant and looking for any difference in the incidence of exit is not very satisfactory in that condition. In Table 4, however, we present such data for the discussion and no-discussion versions of the high-exit incentive condition where exiting was more frequent. We have grouped expectations into the two broad orderings of the three payoffs in that condition ($E > D > C$ and $D > E > C$), because behavioral predictions for cooperators and for defectors are defined by those two orderings. If

Table 4. The Incidence of Exiting, by Choice and Condition with Expectations Constant, High Exit Incentive

	Condition			
	Discussion		No Discussion	
Expectations[a]	Cooperators	Defectors	Cooperators	Defectors
0-49.9%	–	1/3 (33%)	0	15/33 (45%)
+50%	13/61 (21%)	2/7 (29%)	4/10 (40%)	4/8 (50%)

[a] We omit the choices of subjects whose expectations left them indifferent between the exit option and staying—for this condition, only those defectors who expected exactly 50% to cooperate.

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[15] The latter is shown by the T and p values in Table 3. The former is shown here. For cooperators in discussion conditions the average is 8.43 ($N = 110$) and in no discussion conditions the average is 6.79 ($N = 24$). The corresponding T value is 6.38 with $df = 132$ and a $p = .000$. For defectors in discussion conditions the average is 6.09 ($N = 23$) and in no discussion conditions the average is 3.10 ($N = 89$). The T value is 7.73 with $df = 110$ and a $p = .000$.

cooperators and defectors are acting in a dollar-rational manner with respect to exit, and if differentially skewed expectations account for our findings, then we should see greater exiting among cooperators within expectation categories. But we do not; in fact, the differences that exist are in the opposite direction, with more defectors exiting within expectation categories than cooperators. There is nothing here to support the hypothesis that different expectations about what will happen in the group account for the failure of cooperators to exit in greater proportions than defectors.

Looking, then, at assumption 3 we can hypothesize:

Cooperators are not acting wholely on the basis of dollar rationality when it comes to making their decision between staying and exiting. They are (contrary to assumption 3) motivated to some extent at least by a concern for the welfare of the group.

Cooperators have, after all, made one decision—to cooperate—that is readily interpreted as based on a willingness to accept some personal cost in the interests of group welfare. If, as we have suggested above, there are second order externalities associated with the staying and leaving alternatives, then it seems plausible that a concern for such effects will also constrain their exiting behavior. We can speculate, of course, about the power of such a constraining influence compared to the power of concern over first order externalities, but it is clearly possible that what power does exist could account for the behavioral patterns we have reported. The exit-vs.-stay decision might not be free of ethical implications, at least for cooperators.

But how can we observe the values that underlie decisions such as this? There are problems with inferring motivations from behavior because, obviously, a given behavior might be prompted by a variety of motivations. And there are problems with inferring motivations from subjects' own explanations for that behavior because, just as obviously, such explanations might be distorted by rationalization, projection, misunderstanding of self, and a variety of similar processes.

But in the present case it is possible to get some reasonably strong inferences from a combination of behavioral observation and simply asking subjects the reason for what they did.[16] We are testing the hypothesis that the staying behavior of some cooperators, at least, is knowingly against their personal dollar interest and is motivated instead by a concern for the welfare of the group. Although we have no direct way of telling whether subjects believed their staying decisions were against their personal dollar interest—we did not ask them that directly—we can categorize them according to whether their expectations about what others would do made their observed behavior rational or irrational in dollar-maximizing terms,[17] and then examine the explanations for behavior given by subjects who were dollar-rational and dollar-irrational. Any substantial difference between behavioral categories in the explanations given for behavior ought to be accorded at least prima facie credibility.

Unfortunately, there are again too few exiting subjects in the low-exit-incentive conditions to permit confident inferences, but numbers are not a problem in the high-exit-incentive conditions, and we will confine ourselves to them. Thus, for the high-exit-incentive games, defecting subjects are making dollar-rational exit-stay decisions if they exit when fewer than 50% are expected to cooperate or if they stay when more than 50% are expected to cooperate; and they are making dollar-irrational exit-stay decisions if they stay when fewer than 50% are expected to cooperate *or* if they exit when more than 50% are expected to cooperate. Cooperating subjects are making dollar-rational decisions whenever they leave, regardless of their expectations; and they are making dollar-irrational decisions whenever they stay, regardless of their expectations. (This latter is, of course, a consequence of the fact that the expected value of exiting is greater than any available cooperate payoffs for these games.[18])

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[16] Barry (1970, p. 90) quotes with approval the following from Blake and Davis (1964):

> the difficulty of proving the existence of a norm is great. As a consequence there is a tendency to take regularities in behavior as evidence of the norm. When this is done, to explain behavior in terms of the norm is a redundancy. Seen in this light, statements such as the following are redundant: "Knowledge of a culture makes it possible to predict a good many of the actions of any person who shares that culture." Why not simply say: "Knowledge of behavior in a society makes it possible to predict behavior in that society."

But, as Barry goes on to argue, there is nothing logically wrong with inferring the existence of values or norms from the behavior of responding to a questionnaire.

[17] The decision form on which subjects recorded their cooperate v. defect choice asked them about their expectations about the choice of each other subject in the experiment. (Subjects were identified by letters from A through G.)

[18] We are not directly concerned with the important problem of just how players come to hold their expecta-

Table 5. Dollar Rationality among Cooperators and Defectors, High Exit-Incentive Conditions[a]

	Discussion		No Discussion	
	Cooperate	Defect	Cooperate	Defect
Dollar-rational	10	6	4	15
Dollar-irrational	48	3	6	16
	Chi square = 10.470		Chi square = .214	
	$p = .001$		$p = .64$	

[a] These data exclude respondents whose responses to the expectations questions did not permit categorization in terms of dollar rationality or dollar irrationality. In this case dollar-rational cooperators always left, and dollar-irrational ones always stayed. Notice that the figures here vary somewhat from those in Table 1 for the incidence of staying and leaving. This is because of our decision to define dollar-rationality and dollar-irrationality on the basis of subjects' estimates of others' behavior rather than on their own behavior. Not all subjects gave usable estimates, which may reflect on their own capacity to act in a dollar-rational manner.

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tions about the behavior of others in the game. The standard assumption in economic theorizing is that expectations are explained exogenously, or perhaps as a consequence of structural variables of one kind or another. But Dawes et al. (1977) present data suggesting that expectations are, at least in part, a consequence of the player's own cooperate-defect choice—they can be explained as projections from one's own decision. Their finding was made in the context of a standard binary prisoners' dilemma game in which the defect alternative was unambiguously dominant and for which, therefore, expectations about others' behavior would be not formally relevant to the choices of dollar-maximizing players—meaning that it should be generalized to the expectations-behavior problem in other games only with caution. It does, however, constitute a caution for the standard assumption. In the present case, a behavioral discontinuity with expectations is sufficient to categorize a subject as dollar-irrational, regardless of the direction of causality.

As Table 5 shows, dollar-irrationality is not the particular preserve of either cooperators or defectors in the absence of group discussion, but it is significantly associated with cooperation in the presence of group discussion. When group members have an opportunity to talk among themselves about the decision problem they face, cooperation substantially increases even though no expectations about others' behavior could make that behavior dollar-rational.

To the point of the hypothesis, in Table 6 we present the explanations for staying and leaving that were given by cooperators and defectors who were, respectively, dollar-rational and dollar-irrational. Explanations were recorded in an open-ended manner, and initial coding categories were developed according to responses given. For the present purposes, these initial categories were re-grouped with an eye to our theoretical interest

Table 6. Explanations for Staying and Leaving, by Choice and Condition, High Exit Incentive Conditions

	Discussion				No Discussion			
	Cooperate		Defect		Cooperate		Defect	
	I[a]	R[b]	I	R	I	R	I	R
Stay								
Dollar-maximizing	5	–	–	4	1	–	4	3
Risk related	15	–	–	1	1	–	6	–
Group regarding	24	–	–	–	3	–	–	–
Other	3	–	–	1	1	–	3	1
Leave								
Dollar-maximizing	–	7	1	–	–	1	1	3
Mistrusts group	–	3	1	–	–	1	1	7
Other	–	–	1	–	–	2	1	1

[a] Dollar-irrational.
[b] Dollar-rational.

in the difference between dollar-rational and dollar-irrational behavior.[19] Of greatest interest are the responses given by dollar-irrational cooperators (all of whom, by definition in this high-exit-incentive game, were stayers) in the discussion condition where numbers are sufficient to accord some stability to estimates. Notably, 24 of 47 (51%) offered explanations categorizable as group-regarding, whereas only 5 (11%) offered explanations categorizable as dollar-maximizing. Even if the 15 (32%) who indicated that they stayed for risk-related reasons are conservatively interpreted as being concerned to capture more dollars from a relatively predictable group than from a risky exit option, this does represent a substantial expression of group-regarding motives. The credibility of such expressions is increased in addition by the happy expressions of dollar-maximizing concern that came from the relatively few cooperators and defectors whose decision to exit was, in fact, categorizable as dollar-rational. In this condition—where a cooperator's decision to stay was never dollar-rational—the cooperators who stayed appear to have been, very often, motivated by a concern for the welfare of the group rather than by a concern for their personal dollar welfare.

A Revised Model of Exiting

On the basis of the above data, therefore, we can reject the model of exiting behavior that is based on the assumption of universal dollar-maximizing behavior and argue as follows. Prisoners' dilemma games exist—must exist—in a context of normative concern about right and wrong behavior, and that concern will produce some measure of cooperation at the time of the binary choice between cooperation and defection; just how much, of course, will depend on such things as the dollar incentives against cooperation (the price of group-regarding behavior), the strength of group-regarding values, and the game circumstances that support them. Some part of the population will be prepared to include first-order externalities in their personal calculus when making the decision between cooperation and defection. But that group-regarding concern will also be reflected, to some extent, in the decision between staying and leaving, and we can expect that those who made a group-regarding decision on the first occasion will be more prone to make one on the second occasion as well.

Not all those who cooperated will stay, of course; the normative implications of the decision to leave are likely to be less pressing than the normative implications of the decision to defect, and some who had been willing to constrain their private maximizing behavior on the one occasion will not do so on the other. In fact, the availability of an exit option might be used by some cooperators as a normatively acceptable device for escaping the costs that their normative scruples would otherwise impose on them. But enough will stay to negate the prediction derived from the assumption of dollar-maximizing behavior.

In these terms, then, defectors are those whose group-regarding concern, if present at all, is not sufficient to promote cooperation. They are more concerned with the private costs associated with cooperation than with the group benefits and, by extension, will be likely to approach the choice between exiting and staying in the same way. Whether or not they decide to stay will depend on what they expect to reap, in dollar or game terms, from staying—which, in turn, will depend on their expectations about the behavior of others (no doubt influenced, to some extent, by their own behavioral dispositions). Remembering that nobody is immune to normative pressures, it is possible that some defectors see the exit option as a way of avoiding the guilt feelings associated with their defect choice; the exit option might thus serve as a reasonable compromise in the tug-of-war between individual interest and group interest for both cooperators and defectors.

Notice that the centrality of normative constraint to this model means, in naturally occurring situations as in experimental ones, that a prior understanding about the structure of normative systems and their strength will be necessary before accurate predictions can be made about the incidence of cooperation among those remaining in the group after the exit option has been exercised. In our experiments we found that cooperators did not leave any more readily than defectors, and we found some evidence, too, that defectors were leaving more readily than cooperators. But this result could be a unique reflection of the normative patterns existing among our particular subject population and cannot be taken as necessarily indicating what would happen with a different population or with different game parameters. The experimental result is significant, we believe, for emphasizing the importance of normative constraint as a factor behind the exit-stay decision as well as the cooperate-defect one and should be taken as showing the need for greater understanding of normative systems rather than as predicting general outcomes. Without an understanding of the constraint emerging from such systems, behavioral predictions cannot be made with confidence.

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[19] Thus, of the reasons for staying, "higher expected payoff," "strategic maximizing, granted expectations," and "simple dominance of the 'stay' payoff" were grouped under dollar-maximizing; "too difficult to calculate; transaction costs," "stays because does not understand what's happening," "rejects risk involved in exiting" and "satisficing—what I can get from my 'stay' strategy is good enough" were grouped under risk-*related;* and "rejects money incentive; cooperators will stay," "trusts group; wants cooperator-group solidarity in staying," "trusts group—but also recognizes self-interest," "prefers group dependency to lottery; confident in the group's decision," "you have to trust, have faith," "responds to group pressure to stay," "the group decided this way and I am going to do it accordingly" (discussion conditions only) and "cites 'fairness' and other values" were grouped under group regarding.

Of the reasons for leaving, "higher expected payoff," "strategic maximizing, given expectations," and "responds to dollar amounts in the matrix" were grouped under dollar-maximizing; "prefers lottery to group dependency," "prefers lottery in general" and "mistrusts group in general" were grouped under mistrusts group.

What implications can be drawn from the considerable power of group discussion in promoting not only cooperation, but also staying behavior among cooperators? Prominent in our discussion sessions were assertions and promises that individuals would cooperate and, with due allowance for normal skepticism about such things, that would seem to predict an increase in dollar-rational staying behavior among both cooperators and defectors; the more cooperation is expected, the higher are the dollar payoffs to both cooperators and defectors. But the interesting finding has to do with the apparent capacity of group discussion to promote dollar-irrational staying behavior among cooperators, and the best explanation for that seems to be in terms of discussion's capacity to promote normative constraint and group-regardingness. We did not hear much explicit invoking of cooperation norms such as fairness and right behavior, but we did hear considerable effort to clarify the consequences for oneself and for other members of the group of cooperating or defecting, as well as of staying or exiting, and such clarification would seem to be a necessary condition (albeit, not a sufficient one) for normative constraint to work. If subjects were concerned about the consequences of their actions for others, then group discussion could well have served to promote that understanding and to demonstrate the relevance of their normative scruples to the choices at hand. Generally, however, anything that serves to increase normative constraint seems likely, not only to increase cooperation, but also to increase the propensity of cooperators to stay in the game.

Conclusions

In 1970, Albert Hirschman alerted political scientists to the importance of the exit option to what happened in a community or group (economists, as he suggests, were already aware of that and needed to be reminded about the importance of voice), but he did not talk explicitly in terms of the prisoners' dilemma. Similarly, prisoners' dilemma research (both formal and empirical) has been conducted almost exclusively in terms of a binary model in which players are confronted only with the cooperate and defect choice and are, effectively, locked in the game. In this article we have attempted to bring these two traditions together. Although there are, clearly, some prisoners' dilemmas in which (effectively) there is no exit opportunity for the players, there are many others in which there is, and we believe those situations should be modeled in trinary-choice terms. In this article we have developed predictions for the trinary case based on the assumption of dollar-maximizing behavior from players, and we have tested those predictions with small-group experimental data. Those predictions were not met, and we have advanced a further model by which normative constraint (or group-regardingness) serves not only to produce cooperation, but also to keep it in the game once it has been produced.

The opposite side of the exit problem is, of course, the entrance or joining problem. What kind of people will most likely be attracted to group membership? Perhaps the logic of the present model can be extended to address that matter, but the issue seems likely to become an empirical one in relatively short order. Is it possible that cooperators will be selected in by the availability of an entrance option? How important will expectations about what is likely to happen in the group be to the decision to join or not to join? Will potential group members use their current choices to predict their likely payoffs in new groups? We have argued that normative constraint inhibits the exiting decisions of cooperators, but what role—if any—will normative constraint play when people are not yet members of a group?

Appendix 1

	Number choosing 0	Number choosing X	Payoffs to those choosing 0	Payoffs to those choosing X
Eight person matrix	0	8	–	$ 5.00
	1	7	$ 1.25	6.25
	2	6	2.50	7.50
	3	5	3.75	8.75
	4	4	5.00	10.00
	5	3	6.25	11.25
	6	2	7.50	12.50
	7	1	8.75	13.75
	8	0	10.00	–
Seven person matrix	0	7	–	$ 5.00
	1	6	$ 1.50	6.50
	2	5	3.00	8.00
	3	4	4.25	9.25
	4	3	5.75	10.75
	5	2	7.00	12.00
	6	1	8.50	13.50
	7	0	10.00	–
Six person matrix	0	6	–	$ 5.00
	1	5	$ 1.75	6.75
	2	4	3.25	8.25
	3	3	5.00	10.00
	4	2	6.75	11.75
	5	1	8.25	13.25
	6	0	10.00	–
Five person matrix	0	5	–	$ 5.00
	1	4	$ 2.00	7.00
	2	3	4.00	9.00
	3	2	6.00	11.00
	4	1	8.00	13.00
	5	0	10.00	–
Four person matrix	0	4	–	$ 5.00
	1	3	$ 2.50	7.50
	2	2	5.00	10.00
	3	1	7.50	12.50
	4	0	10.00	–
Three person matrix	0	3	–	$ 5.00
	1	2	$ 3.25	8.25
	2	1	6.75	11.75
	3	0	10.00	–
Two person matrix	0	2	–	$ 5.00
	1	1	$ 5.00	10.00
	2	0	10.00	–
One person matrix	0	1	–	$ 5.00
	1	0	$10.00	–

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A Method of Estimating the Personal Ideology of Political Representatives

RICHARD T. CARSON
University of California, Berkeley

JOE A. OPPENHEIMER
University of Maryland

We suggest a method for estimating a political representative's personal ideology and its effect on his or her voting decisions. The current practice of using the ratings of a pressure group such as Americans for Democratic Action (ADA) as a proxy for personal ideology is shown to have a number of theoretical and interpretive flaws. Our technique uses the residuals from two regression equations to provide answers to two questions: Is there a systematic ideological component to the voting behavior of political representatives after taking account of other political determinants, and if a systematic ideological component exists, is it possible to determine its role in voting on particular issues? The technique developed and the currently accepted practice are compared using votes on labor issues as an empirical example.

Legislative voting has long been a subject central to modern American political science. Recently, economists have also become active in this field of study. Over the years a number of alternative models have been proposed to explain legislative voting, and a rough consensus has developed on the identity of the major determinants of voting (Kingdon, 1977). In most models ideology plays a role in determining the legislator's voting pattern. The ensuing pages attempt to convince the reader both that there is something wrong with the way ideology has usually been measured and that our procedure constitutes a substantially improved measure for ideology. The argument of this article is consistent with virtually *any* theoretical model of legislative behavior which gives ideology a place.

To illustrate and test our procedure, it will be necessary to apply our argument within the context of a specific theoretical approach to legislative voting. Our applications are to models that picture the representative's voting act as a political act in which a career politician attempts to further his security, and his stature, by strategically choosing how to vote.[1] Such a model assumes that the individual legislator is motivated by whatever gives electoral, or more broadly, political, security.[2] But such a political view of the legislator is incomplete, for legislators also have personal views and values which enter into their voting decisions (cf. Wittman, 1977).[3]

Normally legislative voting is seen as a function of the representative's political and ideological constraints, i.e., the representative's party, the

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[1] One can point to some of the alternative models put forward in the congressional voting literature. For example, it has been suggested that representatives vote by taking cues from their colleagues, and in particular it has been shown that representatives tend to vote with both their state colleagues and others with whom they find themselves in ideological agreement (Kingdon, 1977; Weisberg, 1978). Other variables that are often considered stem from the Washington, or more specifically, the legislative environment and characteristics of the legislator's electoral coalition (Kushner, 1975; Markus, 1974). Such analyses may indeed be correct and not inconsistent with what we are proposing. Quite the contrary, some shall be shown merely to require a more complex system of equations for tests.

[2] A number of analysts have argued this position with varying degrees of dissent from its assumptions. See, for example, Chappell (1981a, b), Kau and Rubin (1979a, b), Mayhew (1974), and Silberman and Durden (1976).

socioeconomic (or political) characteristics of the legislator's constituents and other supporters, and the legislator's own ideology.[4] In many of these studies, there appears to be considerable agreement on how to measure ideology. Ideology is usually operationalized by the use of one or another of a number of "ratings" by pressure groups. (See, for example, Bernstein & Horn, 1981; Chappell, 1981a, b; Kalt, 1981, 1982; Kenski & Kenski, 1980; Lopreato & Smoller, 1978; MacRae, 1970; Matthews, 1960.) Our argument is not for or against any one particular paradigm, nor with the inclusion of ideology as a determinant of the incumbent's voting patterns, but rather with the *method* used to measure ideology.[5]

Pressure group "ratings" are usually calculated simply as the percentage of the time the legislator voted "with" the pressure group on a specific subset of roll call votes during a particular session. Thus, part of the incumbent's voting record is used as an indicator of ideology. Ideology is then used to explain voting on some other matters. This method creates difficulties of interpretation because we are explaining a set of votes (e.g., on labor issues) by a (perhaps disjoint) second set of votes which are taking place over the same period of time and are theoretically simultaneously determined by the same factors. More generally, the measure of the independent variable, ideology, is itself explained by the same variables as is the dependent variable. For example, if one were interested in explaining voting behavior on, let's say, labor issues, one could estimate COPE[6] scores as a function of the individual's party (P_i), district (D_i), personal position within the legislature (LP_i), and ideology (I_i).

Now COPE scores are highly correlated with such standard measures of ideology as the ratings bestowed on members of Congress by the Americans for Democratic Action (ADA) and by the conservative pressure group, the Americans for Constitutional Action (ACA).[7] Further, since the ADA score, or another proxy for ideology, is simultaneously determined with the dependent variable (COPE), other problems may follow. For example, using ADA as a measure for ideology could cause the other variables to be counted twice—once directly and once through their role in determining ADA. Thus, it is obvious that while the ADA rating (ADA) is correlated with I (and hence a possible proxy for I), it is also correlated with everything else in the equation, giving rise to problems of extreme multicollinearity (and its associated problems—large standard errors in parameter estimates and unreliable tests of significance). Thus, Fiorina (1979) noted, in a critique of an article that used this practice:

> The explanatory variable, liberalism, is measured by the Americans for Democratic Action index. Since the roll call votes that compose the latter are presumably functions of party and constituency as well as ideological influences, it is difficult to treat the summed score as a measure of liberalism alone but . . . (the authors) show no great concern over this (p. 50).

Therefore, when the researcher correlates (for example) an individual's ADA rating with his or her voting on some other issues and reports that correlation as indicating the role of ideology in the incumbent's voting, the ideology is given weights which, at least, are noninterpretable and probably are vastly overstated. Other variables are also assigned peculiar weights.

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[3] There is, however, a tradition, especially in the public choice school, to discount (totally) the personal values of the politicians (e.g., Downs, 1957). The results reported in the fourth part of this article can be seen as a (disconfirming) test of this hypothesis.

[4] The factors that are indicated are usually part of the explanatory variables utilized in legislative voting studies. See, for example, Lopreato and Smoller (1978). Note that it could be that voting is not determined by district characteristics, but rather only by those elements (of or beyond the district) which are selected to be in the legislator's coalition. Note that the coalition would filter both the voters and the interest groups which support the legislator. This is the implication of the work of Markus (1974) and Kushner (1975) and is supported by various public choice models such as that by Oppenheimer (1975).

[5] Usually the personal ideology of a legislator is taken to be reflected in the ratings of that legislator by the Americans for Democratic Action (ADA). But the indices of various groups are highly correlated with each other (which they should be, theoretically, if there is an overall model governing voting behavior) (Poole, 1981). We accept the usual convention of ideology along the liberal—conservative dimension. There are, of course, other dimensions which could be defined. The arguments presented in this article are adaptive.

[6] Here COPE is the AFL-CIO's rating of the legislator. These scores indicate the percentage of the time that the legislator voted "with" the interests of the AFL-CIO on issues selected by the unions.

[7] For example, Kau and Rubin (1979a, p. 369) report the correlations as follows for 1973: $r(\text{COPE,ADA}) = .85$, $r(\text{COPE,ACA}) = -.89$, and $r(\text{ACA,ADA}) = -.90$. Similarly, in our data (see the third and fourth sections below), $r(\text{COPE,ADA}) = .86$.

[8] Ridker and Henning's (1967) problem was that one of their independent variables incorporated the effects of air pollution on property values, the item they wished to measure. Johannes and McAdams develop a residualized ADA variable to measure the representative's deviation from the district's ideological position, as measured by its pro-McGovern vote in 1972.

Indeed, researchers have noticed in their studies that personal ideology was "weighing in" heavily, and that their findings did not reflect normally perceived wisdom (e.g., Bernstein & Horn, 1981; Dunlop & Allen, 1976; Kenski & Kenski, 1980). Theoretically, utilizing ADA as a direct measure of personal ideology double-enters all the district characteristics (once indirectly, via ADA) and consequently makes all the other coefficients less significant than expected. Thus, such a measure poses a number of difficulties of interpretation for the researcher.

Recently, Kalt (1982) grappled with the problem quite directly. In trying to explain "a variable PROCRUDE [which] reflect[s] the frequencies . . . with which [a] senator casts a vote favorable to crude oil producers" (p. 149), he utilizes both the senator's ADA score (as an indicator of ideology), and a few energy related characteristics of the state's economy. Kalt begins the discussion of his results with a warning: "PROADA [the ADA score] might be thought of as the outcome of a stochastic process of the general type that produced PROCRUDE" (p. 155). He probes this possibility as follows: "If there were a systematic relationship between PROCRUDE and the stochastic element in PROADA, a two-stage simultaneous equations technique would be warranted. . . . [But] none of the results at any step of the analysis proved to be at all sensitive to this adjustment. Thus, two-stage results are not reported here" (p. 155).

Kalt was on to the problem. Indeed, he found ideology to be by far the most important variable in the determination of PROCRUDE (1981, pp. 265-268). The problem is not, however, a simultaneous determination of PROCRUDE and PROADA with PROADA being a determinant of PROCRUDE. Rather we conjecture there are underlying factors which are the major determinants of *both* PROCRUDE and PROADA. Kalt can be defended for his procedure since he regressed PROADA on party and the socioeconomic characteristics of the district and realized that PROADA was largely determined by those factors (1981, p. 266). Kalt, who did not conceptualize PROADA as *personal* ideology (1981, p. 260) did not probe the linkage further.

Matthews (1960, pp. 133-135) may have been the first to have sophisticated thoughts about how to rectify these sorts of problems, although he did so with an eye to party loyalty, not ideology. As MacRae (1970, p. 253) puts it:

> Matthews, wishing to measure the *party effort* of individual senators independently of their general liberalism or conservatism, used the method of residuals from a regression line to make this separation. He first plotted the party unity index values, for the senators of a given party, against their conservatism-liberalism scores. . . . He then used the residuals of *party unity*, after the contributions of *liberalism* had been controlled or removed by a regression equation, as a purer index of partisanship (*emphasis added*).

This leads MacRae to conclude (p. 253), "Matthews's reasoning suggests a line of investigation which still remains promising."

Most recently, Kau and Rubin (1979a) and Johannes and McAdams (1981) have built on Ridker and Henning (1967) to "residualize" the traditional (ADA) index of ideology.[8] As Kau and Rubin put it, in their study of how economic influence determines congressional voting:

> To the extent that ADA does measure economic influence, including it in equations where the economic variables are already present may result in multicollinearity. Leaving ADA out would lead to the improper exclusion of ideology. A solution of this problem is to replace ADA with another variable, RADA, that is orthogonal to the economic variables. This new variable is obtained by subtracting from the actual values of ADA the computed values obtained from the ratings equations. The observations on RADA are the residuals. . . . This procedure eliminates multicollinearity between ADA and the economic variables. The coefficients of the economic variables are the same as they would have been had ADA not been included. The coefficient of RADA is unaffected by this procedure. It is of course possible that we have omitted variables from the specification; . . . this problem always exists in empirical work. However, the residualization technique does not amplify this problem. (pp. 370-372)

We shall argue below that with some changes the residualization technique is a useful, standardizable, approach to the problem. We shall apply it to a study of labor votes in the Senate to illustrate its general properties. Note that part of the difficulty with the original measures stems from the non-observable (directly) nature of the *personally held values* segment of the representative's ideology. What is needed is a proxy for this nonobservable segment of ideology, i.e., *personal ideology.*[9] To understand how personal ideology can be measured by the residualization technique, we need to see how using the "residualized" ADA works.

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[9] The problem of unobserved variables has long been recognized as a problem of identification in the econometrics literature (see Goldberger, 1964). For a discussion of treatment of unobserved variables and other statistical procedures used in this article, such as probit analysis and simultaneous equations, from a readable social science perspective, the reader is referred to Haunshek and Jackson (1977).

A Residualized Measure of Ideology

To develop our analysis, consider a representative's vote (V_i) on any issue (i) or group of indexed issues and represent it as a function of party membership (P), district interests (D), legislative position (LP), campaign contributions (C), and personal ideology (I):[10]

$$V_i = f_i(P, D, LP, C, I) \qquad (1)$$

Since I is inherently unobservable, researchers have typically used a proxy variable—frequently the representative's ADA rating.[11] ADA is, however, based on a set of votes and hence is a special case of equation (1). Let us represent that set of votes contained in ADA as V_A; now the typical estimated determinants of the voting equation is

$$V_i = f_i(P, D, LP, C, f_A(P, D, LP, C, I)) \qquad (2)$$

The problem with this representation is obvious. Trying to estimate a vote as a function of another vote produces a situation where none of the coefficients obtained is interpretable. This problem is not solved by using the ADA rating from another year, nor by purging the ADA index of certain votes, since membership, district interests, and personal ideology should be fairly stable over time.[12]

If we grant that an incumbent politico cannot be expected to answer ideological questions in any but a strategic manner, and hence a manner that reflects the political constraints that (s)he is under, there is no independent way to estimate the effect of personal ideology on V_i. However, if we are willing to accept a "stricter" definition of personal ideology, it is possible to derive an acceptable proxy. To see this, consider the redefined relationship

$$V_i = f_i(P^*, D^*, LP^*, C^*, I^*) \qquad (3)$$

where P^* is now party membership *plus* that part of the personal ideological structure of the individual representative which is correlated with P. D^* is the district interests plus that part of I which was correlated with D; LP^* are the constraints from legislative position, plus that part of I which was correlated with LP; C^* is the direct effect of campaign contributions plus the effect of campaign contributions through I; and I^* is that part of I which was uncorrelated (or orthogonal) with party, legislative position, campaign contributions, and district interests. This new definition of I^* can be said to be equivalent to the representative's ideological deviation from those ideological interests of party, legislature, campaign contributions, and district.[13]

That is, to estimate I^* we "residualize" ADA. Ignoring LP and C (so as to minimize confusion) for the remainder of this section, let us define a particular linear specification of equation (3) as:[14]

$$V_i = a_0 + a_1P^* + a_2D^* + a_3I^* + e_i \qquad (4)$$

where e_i is a pure white noise term distributed with zero mean and a scalar covariance matrix. Now, if we were to estimate this equation without I^*, we would get

$$V_i = \hat{a}^*{}_{\mathrm{o}} + \hat{a}^*{}_1P^* + \hat{a}^*{}_2D^* + \hat{U}_i \qquad (5)$$

where $\hat{U}_i = \hat{a}_3I^* + e_1$. Note that when I^* is omitted $\hat{a}^*{}_o$ absorbs the mean value of I^*.[15] There is a problem, since P^* and D^* are unobservable. Equation (5) is, however, equivalent to

$$V_i = \hat{a}^*{}_{\mathrm{o}} + \hat{a}^*{}_1P + \hat{a}^*{}_2D + \hat{U}_i \qquad (6)$$

because of the orthogonality conditions imposed between I^* and P, D and e_i. As Kau and Rubin (1979a) put it, "The coefficients . . . are the same as they would have been had ADA not been included" (p. 370).

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[10] Campaign contributions consist of more than money; they include goods, services, and time, all of which are substitutes over some relevant range.

[11] Again, recall how closely related are all the scores of these "ideological" groups (see note 7).

[12] Normally, the use of ADA scores in these studies is modified so as to take out of the votes used in constructing the ADA index those elements that are also part of the votes being explained (so that one is not explaining something by itself). Researchers have in general been careful to take out those votes that they are trying to explain from those that are used by the group in the construction of the ratings. See, for example, Lopreato and Smoller (1978) and Bernstein and Horn (1981). Kau and Rubin (1979a) carry this one step further. They use ADA (residualized) of one year to explain voting in a second year. This practice does nothing to solve the problem we discuss here, unless the following assumption holds: the error terms between years are uncorrelated. We make this assumption explicit.

[13] The tie between this notion and that developed by Kau and Rubin (1979a, pp. 368-372) and Johannes and McAdams (1981) should be obvious.

[14] The argument is considerably more complex in detail, but not substance, for many common nonlinear specifications of the equations.

[15] Thus, I^* can be redefined as being centered around 0.

Taking the expectation of the observed residual term $\hat{U}_i$ in equation (6):

$$E(U) = E(a_3I^* + e_i) = E(a_3I^*) + E(e_i)$$
$$= E(a_3I^*) + E(0) = a_3I^* \qquad (7)$$

Thus, the residuals from equation (6) contain an unbiased estimate of I^* scaled by a_3.[16] (Since the scale of I^* is unknown, it is of no consequence that a_3 is unknown.)

Our three newly defined variables have new interpretations, as indicated above. For $a^*_1P^*$ and $a^*_2D^*$ incorporate the direct effects of party and the indirect effects of ideology (I) which work through P and D respectively, whereas a_3I^* represents only the direct effects of personal ideology: independent of P and D.[17] The reader should be careful to note that since a_1 and a_2 are estimates of the direct *plus* the indirect effects, they are *biased estimates of the direct effects.*[18]

But there still are problems with using the residuals from equation (6) as a proxy for I^*. First, even though the expectation of every e_{ij} is zero, it gives us little comfort that this is *true on average.* There is no reason to believe that the error term associated with any particular representative, in any particular year, will be zero. If we take just one measurement of our residualized ADA, we cannot tell what is being measured: noise or ideology!

We ought to develop a means of estimating the relative magnitude of I^* to e_i in $\hat{U}_i$. To understand this problem, recall that we wish to extract a measure of the representative's personal ideology which we can utilize as an explanatory variable concerning other issues. Imagine that we extract our measure, enter it into a second equation (to explain some other votes), and discover that the coefficients on the residuals from equation (6), suggested here as our proxy for personal ideology, is not significant in the second equation. There are two possible reasons: the residuals from equation (6) did not contain a systematic component (i.e., I^*), or I^* was not a determinate of the particular issue.[19] Now of course it is preferable to be able to identify which of these two things is going on in the data. If one does not do this, it is difficult to identify in which of the issues ideology does play a part.[20] Thus, our first problem is to find a method to tell us if we have a systematic component in our residuals. To do this, we must discover what is in the residual. We then may be able to use this information to understand better the behavior of representatives.[21]

To sketch the justification of our solutions to these questions, we shall expose the technical roots of our argument. These roots permit identifying the assumptions necessary to justify using the residuals to measure the nonobservable variable. The discussion shall also permit us to interpret our results.[22]

*Matrix Algebraic Analysis of the Approach

Chamberlain (1977) has developed a sophisticated analysis of the "residualization" method of dealing with unobserved variables in a simultaneous equation framework. His model, "assumes that the common left out variable is the only thing connecting the residuals from these equations, making it possible to extract this common factor and control for it" (p. 241). Quite clearly, this is our problem. We estimate a restricted version of Chamberlain's "model 1" and develop our technique within his framework (cf. Chamberlain's model 1, 1977, pp. 243-245, 248-250). Here we shall be careful to distinguish between restrictions we make to identify and apply Chamberlain's model and to simplify our presentation.

To begin, consider three variants of equation (4):

$$V_1 = a_0 + a_1P + a_2D + a_3I + e_1$$
$$V_2 = b_0 + b_1P + b_2D + b_3I + e_2$$
$$V_3 = g_0 + g_1P + g_2D + g_3I + e_3 \qquad (8)$$

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[16] In essence, this is what lay behind the Kau and Rubin (1979a) "residualized" variable, RADA.

[17] Note the analogy between these interpretations and path analysis.

[18] It is not possible to solve for the coefficients of the direct effects without making assumptions about the covariance between *I, P,* and *D.*

[19] Note that this is a real possibility. Consider, for example, the findings of Chappell (1981a) that ideology did not effect voting on cargo bills.

[20] Of course if a significant coefficient is obtained as in most of Kau and Rubin's (1979a) cases, it is safe to assume that the residuals contained I^* and that I^* was a determinant of the issue being examined.

[21] Assume, for the moment, that we solve this problem. It could still be that in $\hat{U}_i$, e_i is very large relative to I^*. Then, $\hat{U}_i$ would be primarily random noise. In such an extreme, a representative may either have no personal ideology or may have one which does not deviate from the ideological interests of party and district. We shall not be able to differentiate well between these two possibilities, in part because of our utilization of a measure of ideology which is "purged" of indirect effects.

[22] Those who are not interested in the technicalities may skip this and other subsection(s) marked with asterisks.

It will greatly simplify the exposition without affecting the analysis or results if we assume that the constant terms (a_0, b_0, and g_0) and the coefficients of P (a_1, b_1, g_1), are zero. Now let us write equation (8) in the standard simultaneous equation framework, $TY = BX + W$. Here,

$$\begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{bmatrix} \begin{bmatrix} V_1 \\ V_2 \\ V_3 \end{bmatrix} = \begin{bmatrix} a_2 & a_3 \\ b_2 & b_3 \\ g_2 & g_3 \end{bmatrix} \begin{bmatrix} D \\ I \end{bmatrix} + \begin{bmatrix} e_1 \\ e_2 \\ e_3 \end{bmatrix} \qquad (9)$$

For Chamberlain's model 1, it is necessary to make restrictions on the T matrix and the error covariance matrix, W, which we shall define shortly.[23] Chamberlain requires T to be upper triangular (or equivalently, lower triangular) and to make enough restrictions on the other elements of T and W to identify the system of equations. Here, we make the most restrictive assumption about T, that it is a diagonal matrix (i.e., all of the nondiagonal elements are zero). This is equivalent to saying that the vote on one issue does not affect a vote on another issue and implies that a vote on an issue does not affect district interests or personal ideology. These restrictions are unnecessary and they are testable.[24]

Next, we turn to the B matrix and the X vector. Note again, I is not observable.[25] Rewriting equation (9) to take I's nonobservable nature into account we have:

$$\begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{bmatrix} \begin{bmatrix} V_1 \\ V_2 \\ V_3 \end{bmatrix} =$$

$$\begin{bmatrix} a_2 \\ b_2 \\ g_2 \end{bmatrix} [D] + \begin{bmatrix} a_3 \\ b_3 \\ g_3 \end{bmatrix} [I] + \begin{bmatrix} e_1 \\ e_2 \\ e_3 \end{bmatrix} \qquad (10)$$

As noted earlier, however, this is equivalent to

$$\begin{bmatrix} a_2 \\ b_2 \\ g_2 \end{bmatrix} [D] + \begin{bmatrix} U_1 \\ U_2 \\ U_3 \end{bmatrix} \qquad (11)$$

where $U = \begin{bmatrix} a_3 \\ b_3 \\ g_3 \end{bmatrix} [I^*] + \begin{bmatrix} e_1 \\ e_2 \\ e_3 \end{bmatrix}$

and $\hat{a}^*_2$, $\hat{b}^*_2$, $\hat{g}^*_2$ are the estimated coefficients which would exist if D^* were the only independent variable.[26] The formulas given by Chamberlain (1977, p. 245) may be used to solve for a_3, b_3, and g_3. In our case, we are only interested in one coefficient for I^*, say g_3. With the restriction we have imposed on the T and W matrices, we no longer have a truly simultaneous equation system, although it does no harm to estimate it as one.

Stage 1. Obtaining I^* from the Residuals to Apply to a Second Stage

A shortcut allows us to answer the questions we posed earlier about using the residuals as an estimator for I^*: "What is in the residuals?" If two or more equations are available, a simple test of the relative magnitudes of the variance of the random error terms is available.[27] To see this, we must first estimate the equations and obtain the residuals, $\hat{U}_i$. Do this for two equations so that $\hat{U}_1 = \hat{a}_3 I^* + e_1$ and $\hat{U}_2 = \hat{b}_3 I^* + e_2$.

What happens if we calculate the correlation coefficient between $\hat{U}_1$ and $\hat{U}_2$?[28] At the extremes, obviously if $r = 0$, I^* is a constant (and hence does not explain differences in voting patterns), and if $r = 1$, e_1 and e_2 have zero variances, and the observed residuals are composed of only I^*.[29] How large r must be in order for the residuals to be a useful proxy is a decision for the individual researcher.[30]

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[23] Chamberlain's other models are developed for cases where alternative types of assumptions might be more appropriate. His model 2 requires restrictions on B and W. His model 3 requires restrictions on T and B as well as W.

[24] One could utilize a simultaneous equation model, similar to those of Kau, Keenan, and Rubin (1982) and Chappell (1981a) to test these assumptions, within our framework. We shall not do this here, however.

[25] $I = I^1 + I^*$ where the orthogonal decomposition desired is such that cov $(I^1, D) \neq 0$ and cov $(I^*, D) = 0$. Either I^1 or I^* or both may be zero vectors. We make the further standard assumption that I is nonstochastic and hence uncorrelated with the stochastic error terms e_1, e_2, and e_3.

[26] Recall that $D^* = (D + I^1)$. Note also that equation (11) is now estimatable as a system of Zellner seemingly unrelated regression equations, and no additional restrictions (given our T) are needed. It makes our situation much easier, however, if we restrict the covariances of $e_1 e_2$, $e_1 e_3$, and $e_2 e_3$ to be zero. Given these assumptions, the expectation of error covariance matrix can be calculated, as can the coefficients of I^* when regressed against ADA.

[27] This test depends on our assumption that the error terms are not correlated across equations (see note 12). $\hat{U}_1$ and $\hat{U}_2$ may be seen as instrumental variables for I^*.

[28] Note that if there are more than two equations, it is possible to add the residuals together and average them first in order to perform the test.

[29] This approach also could be applied to calculate the correlation of the coefficients of I^* in the observed residuals (if they were to be used as an independent variable in a second stage of a multi-stage model) and also to an estimate of the ratio of a_3 to b_3.

Now we have two different residuals, each of which contains I^*. How do we maximize the validity of our measure? If we average these residuals, we get $1/2.(a_3 + b_3)I^* + e_1 + e_2$, and this average minimizes the effects of a large (or peculiar) outlier in any one observation (see Chamberlain, 1977, pp. 249-250). If we had a large number of equations from which to take the observed residuals, it would be reasonable to assume that the sum of the error terms would be zero for all representatives. This is analogous to the test-retest concept familiar in the psychological literature where the observed test score is equal to the true test score plus a random error term that cancels out in repeated testing. Thus, the more equations from which the residuals are obtained, the more reasonable the assumption that the error sum equals zero. We shall later measure the personal ideological variable (over the decade of the 1970s) and, assuming it to remain stable, utilize an averaging process to get a measure which minimizes the effects of single outliers and increases the likelihood of the error terms as summing to zero for all observations.

Summary of Stage 1

Thus, we can again summarize the assumptions that are required for this approach to make sense:

1. "The common left-out variable is the only thing connecting the residuals from these equations, making it possible to extract this common factor and control for it." (p. 241, Chamberlain).[31]
2. *T* is upper triangular (or, equivalently, lower triangular), and *T* and *W* are restricted enough to identify the system of equations.

> Here, we made the most restrictive assumption about *T:* that it is a diagonal matrix (i.e., all of the nondiagonal elements are zero). This is equivalent to saying that a vote on one issue does not affect a vote on another issue and implies that a vote on an issue does not affect district interests or personal ideology.

Now how are we to proceed? We wanted to estimate *I,* an unobservable. The best we can do is to estimate I^*, which is the ideological variable, stripped of its "indirect" effects. To find I^* we residualize *ADA*. Beyond this, we must estimate the relative contents of the residual term: How much is noise and how much is I^*? To do this we: First, choose two or more equations where the absolute values of a_3 and b_3 are likely to be large. The most likely candidates are those equations where V_i is an ideological vote or group of votes. Second, we choose two or more equations with similar dependent and independent variables. If most variables are stable over time, estimating the same equation in two or more different time periods immediately suggests itself. We will shortly utilize both of these criteria in developing our research design to probe I^*, but we first summarize the procedure.

> 1. Take two or more iterations of the same theoretical model such that *I* is the only unobserved variable.
> 2. Perform *r,* or $\bar{R}^2$, test to identify the relative magnitude of the variance that one can ascribe to I^* and to the random error terms.
> 3. Obtain the observed residuals from these equations and average them.
> 4. Use the average of the observed residuals as a measure of I^* in stage 2.

In what follows, it is important to recall a few important points made earlier.

The constant term absorbs the mean value of I^*.

The coefficients of other independent variables are the unbiased estimates of the direct effects plus the indirect effects of *I,* but are biased estimators of the direct effects. Therefore, the estimated variances of the coefficients are positively biased estimates of the true variance of the direct plus indirect effects of *I,* which implies that the *t*-test is more conservative than the indicated level of significance. The estimated variances of the coefficients are biased (in an unknown direction) estimates of the true variance of the direct effect coefficients.

If I^* has a significant coefficient, then personal ideology has an effect. The opposite result is more difficult to interpret since it may occur because *I* has no direct effect after being purged of its correlation with other included variables. Hence our test is extremely strict and will have a low power in rejecting the null hypothesis (i.e., that personal ideology is *not* a determinant of voting on a particular issue).

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[30] In our discussion above, regarding *r,* we implicitly assumed that a_3 and b_3 were nonzero and that their sum was nonzero. The necessity of making these assumptions suggests that we need two or more equations where the absolute values of a_3 and b_3 are as large as possible, relative to their standard errors, and that it is better if a_3 and b_3 be of the same sign so there is no chance of their cancelling out.

[31] Of course, *any* variable which is not explicitly entered into the equations to explain ADA *could* account for the statistical results we report here. It is our belief that any critic's suggestion that the residual really represents another variable must be taken seriously. It is also our belief that the burden of proof is the critic's.

An Illustrative Application of the Residualization Technique

Let us apply our method for the extraction of a measure of the direct effects of personal ideology, I^*. To identify I^* we select a set of votes and identify what is left unexplained by the legislator's party, district, and legislative position. Using ADA votes as a simple indicator for overall manifest ideology, we "explain" those votes by a set of political constraints and personal political values. Consider a simple expansion of our linear specification of the model (equation 3) above[32]

$$ADA = B_0 + B^*_1P^* + B^*_2D^* + B^*_3LP^* + (B_4I^* + e_1) \qquad (12)$$

If our hypothesis is correct, the observed residual term (in the brackets) contains personal ideology as well as a random error term. As indicated above, that term does not readily permit interpretation because it does not provide a means of separating out the two components and because the error term can not be expected to be zero for any single observation. So now let us specify the same equation for two different (perhaps multiyear) time periods (see equations 13a and b), t and $t-1$.[33]

Our measure, I^*, can now be developed as follows. We have two equations where I^* appears in the residual term. From equation (12) and the above discussion, consider the following equations:

$$ADA_t = A_0 + A_1P^* + A_2D^* + A_3LP^* + (A_4I^* + e_t) \qquad (13a)$$

$$ADA_{t-1} = B_0 + B_1P^* + B_2D^* + B_3LP^* + (B_4I^*_2 + e_{t-1}) \qquad (13b)$$

where t and $t-1$ will represent even and odd years respectively for our purposes. This is a recursive simultaneous equation system which can be estimated with OLS or GLS, as appropriate.[34]

I^* can now be consistently estimated by regressing $(A_4I^* + e_t)$ on $(B_4I^* + e_{t-1})$ which becomes:

$$B_4I^*_2 = C_1(A_4I^*_1 + e_{t-1}) + e_t \qquad (14a)$$

or alternatively:

$$A_4I^*_1 = C_2(B_4I^*_2 + e_t) - e_{t-1} \qquad (14b)$$

Note that we have now defined two measures of I^*:

$$\hat{I}^*_2 = (\hat{C}_1/B_4)\,(A_4I^*_1 + e_{t-1}) \qquad (15a)$$

and

$$\hat{I}^*_1 = (\hat{C}_2/A_4)\,(B_4I^*_2 + e_t) \qquad (15b)$$

both of which are consistent estimators of I^*. Since we have no knowledge of which one is the most efficient consistent estimator of I^*, we minimize the risk of a wrong choice and allow for counterbalancing error terms by averaging $\hat{I}^*_1$ and $\hat{I}^*_2$:

$$\hat{I}^* = (\hat{I}^*_1 + \hat{I}^*_2)/2. \qquad (16)$$

Now let us turn to an empirical estimation of the model we proposed above. Estimation of voting models requires the use of qualitative limited dependent variable techniques, maximum likelihood probit or logit for single votes and for voting indices, generalized least squares (GLS) with a probit or logit transformation of the dependent variable to correct for known statistical problems with ordinary least squares regression model (Hanushek & Jackson, 1977). We have used the GLS probit (Judge et al., 1980, 1982) because of the desirability of probit's normal error term assumption.[35] The necessary GLS weights, however, create a problem that has not been dealt with earlier: the unweighted residuals from equations (13a) and (13b) need not have a zero mean and may be correlated with the independent variables in those equations. We need to insure the zero mean and the lack of correlation between I^* and the original set of independent variables so as to allow future weighting of these variables (via probit, etc.) to be interpretable; that is, the *unweighted* form (rather than the form generated *after* a probit transform and weights) of the residualized variables must be uncorrelated with the other independent variables. Thus, to use I^* in equation (16), it is necessary to regress the unweighted residuals of equations (13a) and (13b) against the unweighted independent variables in those equations. (Table 1 reports the regression equations for equations (13a) and (13b).) One can then use the resulting residuals as the measures in equations (15a) and (15b).[36]

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[32] Note that we have dropped campaign contributions from our specification of equation (12), owing to the unavailability of detailed campaign contribution data necessary for much of the time period covered by the data we utilize in the next sections. For later years it is available (see Kau et al., 1982, for a sophisticated treatment of 1979 votes and 1978 campaign contributions by groups). Our omission of campaign contributions is mitigated to some extent by the large number of district characteristics we include in our empirical specification. To the extent that campaign contributions are proportional to the strength of included variables, their effects will be absorbed by those coefficients. We would hypothesize two routes for the effects of campaign contributions on ideology: first, a general indirect "purchase" of ideology and second, a general "purchase" of particular votes. The first route is testable by calculating the correlation coefficient between our estimated measure of ideology and contributions, and the second route by entering contributions directly as an independent variable in a regression on the determinants of those votes. Not surprisingly, preliminary research with campaign contributions from later years indicates the second route to be much more important than the first.

[33] In practice, two-year periods are congressional sessions and represent the longest time period that the researcher can be assured of the same representatives serving together. If the coefficients across different congressional sessions are assumed to be stable, different sessions may be "stacked," exploiting the large number of degrees of freedom. We have made this assumption; it is not necessary.

[34] Although we have assumed the independent variables in both equations to be the same, this is not necessary for our argument. Indeed, one could imagine that in the United States the functional relationship would be different even (election) years and odd (off) years.

Tables 1, 2, and 3 report the results of an application of the proposed technique to the measurement of the ideology of U.S. senators during the 1970s. In Table 1 we have regressed many state characteristics against each senator's annual ADA rating, to identify the residual. (This is in direct contrast with Johannes and McAdams, 1981, p. 517, who regress ADA only with the 1972 McGovern vote.) Personal ideology, by hypothesis, is in this residual. In this procedure we have taken what could be described as a kitchen-sink approach to estimating the ADA equation. In general this is an unacceptable practice because the multicollinearity can be so high that little or nothing can be said about the influence and significance of any of the coefficients. However, our purpose here is simply to minimize the sum of squares of the residuals and to guard against leaving out any significant systematic factors which would otherwise become a part of our estimated I^*.[37] Therefore, in this case, no harm is done by this eclectic approach.

We ask the reader to forgive us: we feel that we must indulge ourselves with an interpretation of the above results which has nothing to do with the main thrust of this article. Consider the coefficients on the DELECT and RELECT variables. They appear to be indicating something quite important. It appears, from their sign and size and significance that we can conclude that first, pressures for reelection affect primarily voting in the odd calendar years. Thus it appears that the incumbent campaigns on his previous year's record. Further, and this is of some theoretical importance, the Democrats are pushed to the right, and the Republicans to the left, by the elections. If you wish, during odd years they seem to *converge* (toward some median voter?) whereas during even years (when their votes are perhaps too close in time to be fully digested and considered by the interested public for the election), the parties are further apart and driven more by their natural coalition members. This observation would indicate that any model of party behavior should combine the median voter analysis of Downs along with the coalitional analysis of those cited in note 4 above.

The above regressions leave residuals that (after correcting for the GLS weights), we conjecture to be personal ideology. If our conjecture is correct, these residuals should be related, and we can check the validity of our measure by regressing the residuals of the odd years against the residuals of the even years. Doing this would lead us to expect a sizable correlation between the two sets of years (the larger the correlation, the more stable, and the larger the I^* component in the residuals).[38] We report this test in Table 2. These results show that the observed unweighted, corrected, residuals share a common variance of 36% of their variance.

Yet another test suggests itself, if $B_4 I^*_1 = 0$, or the average of the two residuals (i.e., if people have no personal ideology as measured or if I^*

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[35] The choice between probit and logit is, however, arbitrary for most applications. In either case it is necessary to use a correction factor to prevent exact zeros and ones which are undefined. We used the one suggested by Lindgren and McElrath (1969).

[36] The result of these two auxiliary regressions are not reported here but are available from the authors. The correlation between the two versions of the residuals is approximately .9 for even, and .8 for odd, years.

[37] One could emphasize further the coalitional aspects of the relationship between constituents and voting in the legislature. To do this, one could enter each of the district characteristics twice: once modified by a dummy variable representing party. Such a model would perhaps better represent the works indicated in note 4 above.

[38] The formula for the correlation coefficient clearly shows what is happening, given our assumptions:

$$r = (\mathrm{var}(I^*)/\{\mathrm{var}(I^*) + [\mathrm{var}(e_1)]^{1/2} + [\mathrm{var}(e_2)]^{1/2}\}.$$

Table 1. Estimation of Personal Ideology (U.S. Senate, 1970-1980)

	Dependent variable			
	ADA[b] (even years)		ADA[b] (odd years)	
Independent variables[a]	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
Intercept	−0.029	−0.0	−2.091	−0.8
Party	0.724	9.7	0.802	9.4
New England	0.157	0.7	−0.392	−1.7
East North Central	0.028	0.2	0.601	4.1
West North Central	−0.708	−4.7	−0.130	−0.8
South Atlantic	−0.759	−4.3	−0.695	−3.6
East South Central	−0.534	−2.1	−0.646	−2.2
West South Central	−0.744	−3.2	−0.163	−0.7
Mountain	−0.932	−4.5	−0.244	−1.1
Pacific	−0.511	−2.5	−0.486	−2.2
DElect	−0.086	−1.7	−0.143	−2.7
RElect	0.031	0.3	0.424	4.1
PMcGov	0.029	4.5	0.035	5.2
PCarter	0.028	3.3	0.009	1.0
SREP	0.107	0.7	0.142	0.8
PAFLCIO	0.013	1.6	−0.019	−2.1
PMAN	−0.072	−4.8	0.039	2.5
YOFF	−0.002	−0.5	−0.007	−1.8
PBLACK	0.008	1.1	−0.007	−0.9
MVAGE	0.029	1.1	0.094	3.5
MFINC	−0.000	−0.7	0.000	2.2
MEDUC	0.512	3.7	−0.032	−0.2
PWC	−0.034	−1.5	−0.035	−1.3
PSERV	0.250	6.7	−0.136	−3.6
PFARM	0.004	−0.2	−0.028	1.6
PCC	−0.007	−1.7	0.000	0.0
PSUB	0.015	3.9	0.002	0.6
PFEDS	0.000	2.9	0.000	1.5
PCOIL	−3.659	−3.3	−2.594	2.0
PCNGAS	0.229	1.3	−0.123	−0.7
PCCOAL	2.682	0.2	−4.452	−1.3
PENGROUP	0.924	6.0	0.521	3.4
PCCAUSE	−5.120	−3.7	0.205	−0.1
	$R^2 = .57$		$R^2 = .54$	
		$n = 483$		

[a] Some of the variables are self-evident; those that aren't are: a dummy variable indicating the year a Democrat (Republican) faces reelection: DElect (RElect); SREP: a dummy variable indicating a southern Republican; the percent of the vote for McGovern and Carter (1976): PMcGov, PCarter; percent of the labor force in the AFL-CIO: PAFLCIO; the percent of the labor force in manufacturing: PMAN and farming: PFARM; the incumbent's years in office: YOFF; the median voter's age in the district: MVAGE; median family income: MFINC; median education: MEDUC; percent white collar: PWC; and percent service worker in the labor force: PSERV; percentage of the federal budget received by the state; PFED; the percentage of the state population belonging to 7 national environmental groups: PENGRP; the percent of the population belonging to Common Cause: PCCAUSE; the production of oil, natural gas, and coal: PCOIL, PCNGAS, & PCCOAL respectfully. All of the data are taken from Barone, et al., *Almanac of American Politics,* 1970-1982, except (1) the figures on the membership in the interest groups (Common Cause, the environmental groups, and the AFL-CIO) which were obtained directly from the groups involved and (2) the energy production figures which came from Shreck (1977), pp. 940, 1081, and 397.

[b] Here the actual dependent variable is the PROBIT transformation of the ADA evaluation. See note 35.

Table 2. Regressing the Odd Years and the Even Years Residuals

Dependent variable $e_t + B_4I^*_2$ = Unweighted even year residuals			Dependent variable $e_t + A_4I^*_1$ = Unweighted odd year residuals		
Independent variable	Coefficient	t-Statistic	Independent variable	Coefficient	t-Statistic
Intercept	$5.55 \times E-14$	0.0	Intercept	$-4.44 \times E-14$	0.0
$e_t + A_4I_1$	.567	16.6	$e_t + B_4I_1$	.642	16.6

$n = 483$

$\bar{R}^2 = .36$

plays no role in determining ADA), then adding the observed residual to the equation for the $t-1$ time period as an independent variable should not greatly effect the explanatory power of the equation. After all, adding the residual would merely be adding a random error term. On the other hand, if it *does* contain personal ideology, then it *will* significantly increase the overall explanatory power of the time period t regression.

Performing this test supports our conjecture. We considered the set of senators and their ADA ratings in even and odd years.[39] We found that we could explain 54% (R^2) of the variance of ADA in odd years using *D, LP,* and *P,* and 64% (R^2) of the variance when augmenting those variables with the even years' residuals. Similarly, in even years, the figures are 57% and 68%, for gains in both data sets of a bit more than 10%.

With these strong results, we continue with our investigation. Are the residuals a sensible indicator of the senators' ideologies? To see this we transform the residuals onto a scale from -1 (conservative) to $+1$ (liberal). These techniques are utilized to calculate I^* for members of the Senate in 1979 and 1980, and the results are reported in the next table (Table 3). Although here we perform no statistical checks on these results, one is able to ask whether the results are intuitively plausible.

Our intuition is not fully satisfied with these results. A close examination shows, for example, there are a number of peculiar rankings that must be mentioned. But first, note that we are not accustomed to think of the representative's ideology in this fashion, and hence there are bound to be some seemingly anomalous results. The causes for these anomalies should perhaps be discussed. Putting this aside as nontrivial, and yet not quite to the point, other obvious causes for anomalies come to mind. First, unlike the results given at other points in this article, the scores are calculated only over a two-year period. Hence all the vagaries of small numbers of observations for each person come into play. Consider the case of Williams (D, N.J.) for example. His ADA ratings in 1979 and 1980 were uncharacteristically low. For the years from 1972 to 1978, he averaged an 86 ADA, and had no score below 80. His average would have yielded him a I^* of about $-.46$ (note, still in the most conservative quartile). Second, as indicated at various points above, there is no attempt to take into account the specifically coalitional constraints upon the representatives; that is, we have not tried to develop a model that takes into account the specific local array of interest groups supporting each of the senators. The only time we even approximated this, by considering southern Republicans as separate from other Republicans, we found that they tended to differ from other Republicans. Southern Republicanism was tied (weakly, to be sure) to a more liberal record. Presumably, some of the variance would be explained by such a variable. Finally, there has been no attempt to introduce the *financial* constituents, or backers, of the representatives, as a further constraint to their voting behavior. Were we to follow up on these last two points, the ratings could change quite substantially. But there would still be an unexplained pattern to the anomalies: southern Democrats who were not *very* conservative now appear, by our measure, to be quite liberal. (See, for example, Hollings of South Carolina.) On the other hand, liberal Republicans are given quite a boost for their bucking of the (negative) ideological correlation between Republicanism and the ADA. For examples, note Leahy of Vermont and Weicker of Connecticut. By permitting a coalitional variable to be included, *some* of the anomalous weightings for nonconservative southern Democrats and liberal northern Republicans could well be mitigated, but probably not all. Recall that we are not used to thinking about ideology this way, partially

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[39] By beginning with an odd year, the ADA rating is not affected by the defeat of an incumbent in an election during the period examined.

Table 3. Ideological Scores: U.S. Senators, 1979-1980[a]

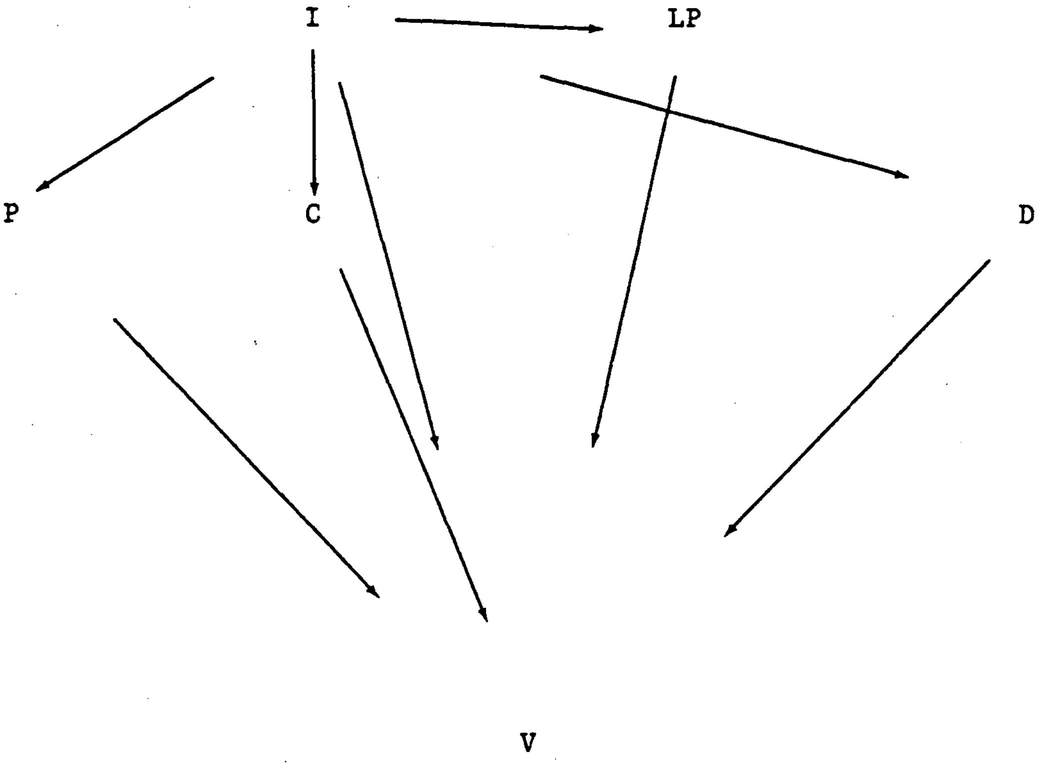
State	Senator	I*[b]	ADA	
			1980	1979
N.J.	*Bradley	−1.03	72	68
Vt.	*Stafford	−1.01	61	47
N.Y.	*Moynihan	−0.89	72	47
Pa.	Schweiker	−0.89	17	16
N.J.	*Williams	−0.82	72	74
Iowa	Jepsen	−0.80	22	0
Conn.	*Ribicoff	−0.75	56	53
Nebr.	*Zorninsky	−0.66	22	21
Mass.	*Tsongas	−0.64	89	74
Minn.	Boschwitz	−0.63	28	21
Ill.	*Stevenson	−0.60	61	68
N.D.	Young	−0.58	11	5
Alaska	*Gravel	−0.56	39	47
Calif.	Hayakawa	−0.54	22	11
Wis.	*Proxmire	−0.51	56	42
Ohio	*Glenn	−0.46	67	53
Nebr.	*Exon	−0.46	39	21
Mont.	*Melcher	−0.44	50	58
Wash.	*Magnuson	−0.42	72	53
Ariz.	Goldwater	−0.40	0	5
Okla.	*Boren	−0.40	28	16
N.H.	Humphrey	−0.37	6	5
Idaho	McClure	−0.32	17	0
R.I.	*Pell	−0.32	78	68
N.H.	*Durkin	−0.27	50	37
Nev.	*Cannon	−0.25	33	21
Idaho	*Church	−0.24	50	42
Fla.	*Stone	−0.23	33	21
Ind.	Lugar	−0.22	17	11
S.D.	Pressler	−0.22	17	32
Del.	Roth	−0.20	22	21
Ky.	*Huddleston	−0.17	44	37
Wash.	*Jackson	−0.17	72	68
Colo.	Armstrong	−0.15	17	11
Ga.	*Talmadge	−0.15	33	16
Ariz.	*Deconcini	−0.13	67	26
Mont.	*Baucus	−0.13	72	63
La.	*Johnston	−0.13	33	16
Oreg.	Packwood	−0.10	56	32
Maine	Cohen	−0.09	33	42
Va.	Warner	−0.08	22	5
Ind.	*Bayh	−0.06	61	63
Minn.	Durenberger	−0.05	44	53
Ga.	*Nunn	−0.04	56	11
Iowa	*Culver	−0.03	78	74
Pa.	Heinz	−0.03	50	42
Del.	*Biden	−0.03	67	53
Ariz.	*Pryor	−0.01	44	42
La.	*Long	−0.00	28	11
N.C.	*Morgan	−0.00	22	26
Colo.	*Hart	0.01	61	58
Ohio	*Metzenbaum	0.02	83	84
Ill.	Percy	0.02	39	47
W. Va.	*Randolph	0.04	72	32
Kans.	Dole	0.04	22	21
Hawaii	*Inouye	0.05	67	37
N.M.	Domenici	0.08	17	5
Nev.	Laxalt	0.08	11	5
W. Va.	*Byrd	0.09	56	53
Miss.	*Reigle	0.11	83	84
Ala.	*Helfin	0.12	39	26
N.C.	Helms	0.12	11	5
Calif.	*Cranston	0.13	83	79
Tex.	*Bentsen	0.14	39	26
Md.	*Sarbanes	0.15	83	79
N.D.	*Burdick	0.16	78	63
Hawaii	*Matsunaga	0.16	78	53
Mich.	*Levin	0.17	94	74
Fla.	*Chiles	0.18	50	42
Tex.	Tower	0.20	6	11
N.Y.	Javits	0.20	61	63
Oreg.	Hatfield	0.22	50	58
Alaska	Stevens	0.22	39	21
Utah	Garn	0.25	17	11
Okla.	Bellmon	0.26	28	16
S.D.	*McGovern	0.27	56	100
N. Mex.	Schmitt	0.28	17	16
Tenn.	Baker	0.28	17	21
Ky.	*Ford	0.28	78	32
Tenn.	*Sasser	0.29	67	37
Mo.	*Eagleton	0.30	78	58
Utah	Hatch	0.30	17	11
Wyo.	Simpson	0.32	17	11
R.I.	Chaffee	0.39	72	47
Ark.	*Bumpers	0.40	56	53
Mo.	Danforth	0.40	50	16
Wyo.	Wallop	0.41	22	11
Miss.	*Stennis	0.42	17	11
Wis.	*Nelson	0.43	89	84
Md.	Mathias	0.51	72	63
Kans.	Kassenbaum	0.53	44	37
S.C.	Thurmond	0.59	17	5
S.C.	*Hollings	0.64	39	32
Ala.	*Stewart	0.67	61	42
Conn.	Weicker	0.71	72	68
Vt.	Leahy	0.89	83	89
Miss.	Cochran	0.89	22	5

*Democrat.

[a] Kennedy (Mass.) was omitted owing to excessive absence resulting from campaign activities, Muskie (Maine) because he resigned to become Secretary of State, and Byrd (Va.) because he is an independent.

[b] The larger the score, the more liberal the ideology. Note that in these "normalized" scores, we have centered the scores around zero (by subtracting the mean). The scores reflect the average for the two years.

Figure 1. The Current Paradigmatic Model to Explain Voting Patterns of Legislators



because I^* is that segment of ideology which is *independent* of party. A reexamination of Table 1 helps to illuminate the issue. Not surprisingly, party and region are extremely important constraints of ADA. Hence bucking these political constraints yields large residuals or I^*. Typically, then, our measure puts strong liberal labels on Republicans who are liberal, and southerners who are not extreme conservatives. Conservative labels go to Democrats who are less liberal than their party-region nexus might lead one to believe. Hence, Leahy, the Vermont Republican, has very little pressure to vote liberal. Both his party and his state push him to the right. An ADA score of 83 reveals him as a great liberal bucking these constraints. Williams, on the other hand, is from a state with demographic, political, and economic characteristics which push him to the left, and he is a Democrat. Thus, his ADA scores in the 70s disappoint a liberal's statistical expectation, and he is labelled a conservative.[40]

Another aspect of our measure of ideology also of interest is its correlation with other standard voting indices. These correlations are: .475 with ADA; .343 with the League of Conservation Voters score; and .327 with COPE.

Stage 2: Explaining Observed Legislative Voting Patterns

Recall the problem: we wish to explain a subset (other than ADA) of the i^{th} legislator's voting record. The general paradigm for the analysis of legislative voting is illustrated in Figure 1. There, party (P), ideology (I), legislative position (LP), campaign contributions (C), and district characteristics (D) are seen as together determining legislative voting patterns. A subset of these votes (call it S) is of interest to us. More specifically, note that V_i can be thought of as follows:

$V_i = Y_j/n$ ($j = 1 \ldots n$) (where $Y_j = 1$ for a yes/positive single vote and $Y_j = 0$ for a no/negative vote; $N =$ the number of votes). V_i can be a single vote or group of votes (for a single vote $Y_j = 1$ or 0 and $n = 1$).

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[40] Note that the (earlier) very liberal record of the Republican, Case, from the same state, merely reinforces this impression.

In other words, elaborating on equation 1 we want to estimate (dropping the i subscript)

$$V(S) = f(P, D, LP, C, I) \tag{17}$$

As indicated above, although this is generally estimated as $V = g(P, D, LP, C, ADA)$, where ADA is used a proxy for the hard to measure I, we have developed an alternative residualized I^*. Thus, we conjecture:

$$V(S) = h(P^*, D^*, LP^*, C^*, I^*) \tag{18}$$

Now the structure of equation (18) is not quite the same as the equations in stage 1. Here the residualization of ideology need *not* be orthogonal to the other explanatory variables if they were not included in the equations used to estimate I^* which allows I^* potentially to have indirect effects. Further, recall that we have a measure of ideology which is "purged" of indirect effects (on ADA).

An Application to Illustrate Stage 2

Finally, let us see the effect of utilizing both our measure of ideology and the "standard" measure. To do this, we report the findings of a preliminary attempt to explain (or predict) voting on issues of concern to organized labor.[41] Here we utilize the evaluation, or rating, of the senators by the AFL-CIO (i.e., their COPE scores) as the dependent variable and develop two models (see Table 4). In one we utilize our measure of ideology (I^*) and in the other we utilize the traditional ADA measure.

Table 4. Comparing the Models Applied to Labor Issues: ADA vs. I* as Independent Variables Explaining Cope Scores (n=967)[a]

Independent variable	Coefficient	t-Statistic	PROB > |T|	Standardized Coefficients
Independent variable: I*				
Intercept	−0.086	− 1.2	0.2219	−7.25 × E−10
Party	0.475	11.3	0.0001	.343
YOff	− .011	− 6.0	0.0001	−.173
PMan	0.003	1.7	0.0863	.052
PAFLCIO	0.029	13.4	0.0001	.420
I*	0.174	4.9	0.0001	.148
R^2 = .25				
Independent variable: ADA				
Intercept	−0.458	− 6.9	0.0001	−3.88 × E−09
Party	0.273	7.6	0.0001	0.20
YOff	−0.008	− 5.0	0.0001	−0.13
PMan	−0.001	− 0.9	0.3640	−0.02
PAFLCIO	0.025	12.8	0.0001	0.36
ADA	0.010	16.1	0.0001	0.43
R^2 = .40				
Without ideology				
Intercept	0.028	0.4	0.6748	2.38 × E−10
Party	0.400	10.1	0.0001	0.29
YOff	−0.011	− 5.9	0.0001	−0.17
PMan	0.003	1.6	0.1019	0.05
PAFLCIO	0.027	12.6	0.0001	0.39
R^2 = .23				

[a] Here the actual dependent variable is the probit transformation of the COPE scores.

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[41] The reader may be interested in contrasting our model with two previous studies which used the standard ADA approach to explain voting on minimum wage legislation (Kau & Rubin, 1978; Silberman & Durden, 1976). Interested readers may also wish to obtain ideological ratings which take into account some coalitional characteristics as well as ratings for members of the House. These can be obtained from the authors.

Now the effect of the operationalization is made clear. When ADA is used, party's effect is decreased considerably, as are the effects of all of the other variables, which can be seen by comparing the standardized weights in Tables 4.1 and 4.2. This result occurs because of the strong correlations between ADA ratings and the other included variables. On the other hand, because ADA ratings are a surrogate for many of the characteristics of the state, replacing ADA by I^* decreases the explained variance. Note that one could reintroduce these demographic and economic characteristics of the state now without fear of their correlation with the residualized measure of ideology.

Along with Robert Cameron Mitchell, we have applied the technique to a second substantive question: legislators' voting on environmental issues (Oppenheimer, Mitchell, & Carson, in press). The parallel results there form an interesting contrast. I^* there is far more effective as an explanatory variable than it is in explaining COPE scores. Using party and the percentage of the state population who are members of environmental groups along with ideology, we found I^* increased R^2 from .16 to .24. Further, it yielded an equation which quite sensibly indicated that all three variables were highly significant (with 479 degrees of freedom, t was above 7 for all coefficients). This was in contrast to the results when ADA was used instead of I^*. ADA, as a surrogate for all the socioeconomic and political characteristics of the state yielded a very improved R^2 (.51). But it so distorted the weights that party's coefficient was significant only at a .07 level. Here again, one can reintroduce the demographic and economic characteristics of the state, with I^*, and without fear that the correlation with I^* will lead to difficulties of interpretation.

Conclusions

We have developed a procedure for extracting a politician's ideological leaning, relative to his or her political environment, which may be interpreted in various ways. If the electoral environment is defined strictly as the politician's electorate, it can be thought of as a deviation from the electoral unit from which he or she is elected, as conceived of by Johannes and McAdams (1981). But if we define the political environment more broadly, there are many possible interpretations of this variable. It could be personal ideology, or purchased positions by forces outside of the incumbent's electorate, or positions taken for national political considerations (e.g., to go along with the presidency), depending upon which variables are brought to bear. All of these interpretations can be tested in their own right. But that would constitute a further article (see Oppenheimer, Mitchell, & Carson, in press). Any of these interpretations would make some politicians who have liberal records look conservative, namely those politicians who score high on ADA ratings, but come from even more liberal electorates. Similarly, some politicians from conservative districts, with less conservative records, will appear liberal. Hence, this method is likely to stretch the theoretical understanding and conceptualization of ideology which we hold.

Let us review the assumptions underlying this technique. First, we assume that the residuals (for example, as calculated in Table 1 above) contain *only* a random error term and our I^* variable. This is a rebuttable presumption. But at this point in time, the burden of proof seems to be on the critic. After all, any variable conjectured to be contained in the residual can be tested for, and the new residual calculated to examine its nature. Thus, the general technique does *not* require that we have correctly isolated I^* at this point. Second, we assume that each individual's personal ideology is stable. This requirement is endemic to the procedure, as is the assumption that the actual error terms are uncorrelated over time (and therefore that their correlation is an artifact of the other variable, I^*, contained in the residual). These assumptions permit the developmment of an alternative measure for ideology which yields a number of benefits including an ability to assess, realistically, the relative importance of the various factors determining a legislator's vote.

The technique has other practical significance. It would permit, for example, interest groups to assess which incumbents are "friendly" and ought to be supported. Rather than identifying those with the highest ADA rating as the most liberal, we now identify those who go furthest to the left of their constituents as the most liberal. This permits us to relativize the voting of the legislators to their constituency and their pressures. Hence we see who is most likely to go out on a limb because of personal political feelings. Similar work could be done for other interest group scores.

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The Particulars of a Universal Politics: Hegel's Adaptation of Montesquieu's Typology

MICHAEL A. MOSHER
University of Washington

This article explores both the influence that Montesquieu's typology of regimes exerted on Hegel's thought and Hegel's ultimate rejection of Montesquieu's monarchical order, especially its cult of honor, as a plausible candidate for containing and expressing the citizen's "claims for private judgment, private willing, and private caprice." Can Hegel provide a substitute model of a polity, neither republican nor despotic, which corrects the particularism of Montesquieu's monarchical society? Marx did not think so. His criticism of Hegel exactly parallels Hegel's criticism of Montesquieu. Hegel's dilemma may be expressed in more contemporary terms as the difficulty in reconciling the goals of "neutrality" and "communicative competence." The latter reflects the concerns of Montesquieu's moral geography, whereas the former reflects the rather different preoccupations of the social contract tradition.

Across the barriers posed by the consequences of Kantian thought and the French Revolution, G. W. F. Hegel found in the Baron de Montesquieu a significant predecessor to his synthetic ambitions. To be sure, Montesquieu's ambitions appear more modest than Hegel's, since they were apparently restricted to the political and social. Within this domain, these philosophers sought comparable goals: to describe a polity suitable to the circumstances of complex and highly differentiated societies, and a polity that might reconcile self-conscious and easily distracted individuals to their political fate. This goal had been accomplished, or so these authors thought, more harmoniously in the politics of antiquity.

As Hegel stated the problem, "In the state of antiquity, the subjective end simply coincided with the state's will. In modern times, however, we make claims for private judgment, private willing, and private caprice" (Hegel, 1952, p. 280, Addition to par. 261).[1] Hegel was not at all persuaded, however, that private men and women could judge well in the absence of those institutional arrangements that lend support to their distracted rationality. Although he believed that his *Philosophy of Right* adequately described those institutional arrangements likely to support the errant rationality (i.e., subjective ends) of dissociated citizens, he also seemed persuaded that the advocates of private judgment would not heed him (Hegel, 1952, pars. 124, 126, 140, 162; 1977, pp. 221-228, 383-409).

Montesquieu's famous typology of regimes can be read as expressing a similar judgment about the circumstances in which subjective temperaments collide in modern polities, although not the same appreciation of the political dangers.

In his typology, Montesquieu combines the structural features of a type of social order (as defined by its legal and social circumstances) with a historical psychology suitable to each structure. Republics (both the democratic and the aristocratic variants) are animated by a range of communitarian sentiments that Montesquieu identifies as simply "virtue." Monarchies are infused with the more dissociative sentiments he calls "honor"; and despotisms are permeated with a climate of "fear" (*Laws* 2-7).

These claims have an epistemological dimension in that each association is structured in accordance with the types and range of understandings between agents it permits or encourages. These settings become in turn the locus for the generation of notions of value and social identity. Against this background the society discovers a suitable politics to frame and sustain these understandings. Montesquieu's imagery of distance suggests the differences among associations. Republics, monarchies, and despotisms are appropriate to small, larger, and excessively large societies (*Laws* 8.20). The "distances" correspond to the degree and type of communicativeness permitted among associated members and not necessarily to the literal size of the polity. Republics permit, if you will, a public life of close encounter. Monarchies encourage only a limited public life but foster at the same time other, more

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[1] Citations to Hegel's *Philosophy of Right* (1952) will sometimes include references to paragraphs abbreviated as follows: par. 261. Similarly, references to *The Spirit of the Laws* (Montesquieu, 1966a) will generally be indicated by the abbreviation *Laws* followed by a book and chapter heading, as in *Laws* 4.2.

privatized relationships among relatively detached and dispersed agents. In despotisms, agents are required to live at vast psychic and political distances from one another. Here the public life has become wholly privatized. Private fearfulness replaces public discourse.

Hegel utilizes these potentially idealist themes in his early works (e.g., in the essay on "natural law"), in the *Phenomenology of Spirit* (1977) and in the *Philosophy of Right* (1952). Montesquieu's typology implicitly frames Hegel's review of specific types of association in the *Phenomenology.* Montesquieu's understanding of a historically evolving community as a relationship of universal and particular and his views upon the requirements of a decentralized structure of decision-making are commended by Hegel in the *Philosophy of Right* (Hegel, 1952, pp. 16, 20, 161, 177-178). Hegel's use of Montesquieu's description of modes of human association suggests agreement with its basic epistemological claims, although the politics that Hegel settles upon these claims is significantly different from that of Montesquieu. Although Hegel is not known for his gratitude toward predecessors, his acknowledgments of Montesquieu nevertheless exhibit this virtue; there are fifteen references by name to Montesquieu in Hegel's corpus.[2] Only one reference suggests criticism.

In this article I explore the implications both of Hegel's intellectual debt to Montesquieu and of Hegel's ultimate rejection of Montesquieu's politics. Hegel's adaptation of Montesquieu's claims concerning the "individuality" of peoples is assessed against the background of Hegel's Kantian project. Then Montesquieu's typology of regimes, identified as his "moral geography," is described from the perspective of an idealist interpretation. Hegel adapts Montesquieu's typology, selectively, to the systems of governments whose evolution and dissolution he analyzes in the *Phenomenology.* Thus, the republic is exemplified as Sophoclean Athens, monarchy is expressed (as it was also for Montesquieu) in the French *ancien régime,* and despotism is demonstrated in the French revolutionary state and, as a significant precursor, in imperial Rome (Hegel, 1977, pp. 266-289, 290-328, 355-363). Hegel's criticism of Montesquieu is then assessed. Hegel's departure from Montesquieu's understanding focuses upon the character of the monarchical order. Hegel seems to agree that its structure captures the dilemmas of dispersed individualists not fortunate enough to live in a republican polity but lucky enough to have avoided despotic politics. Nevertheless, Hegel condemns Montesquieu's description of this society. Its cult of honor is openly rebellious toward authority, and no society, Hegel thought, could survive such anarchism.

Yet the question may be posed: if the monarchical structure is suitable to the "particulars" of a society neither republican nor despotic, can Hegel provide his readers with a substitute for the psychology of honor that does not recreate the "particularism" he evidently abhors? Karl Marx did not think so. His criticism of Hegel's universal class focused precisely on its fraudulent claims to having a universal appeal. This class was also mired in particularity. After discussing Marx's and Hegel's parallel criticism of their predecessors, I assess in a concluding section the kinds of theoretical dilemmas which emerge from these disagreements. I suggest with allusions to Habermas, Oakeshott, Ackerman, and Dworkin, that the goals of "communicative competence" and the "neutral" state form a tempting but uneasy alliance, and that Hegel's problem, expressed in this contemporary vocabulary, follows from his inability to harmonize the demands of both goals.

My predecessors in recording this lineage of ideas have been numerous. Hildegaard Trescher (1918) argued that Montesquieu was a half-way house on the way to Hegel's historicism. Friedrich Meinecke (1972, p. 106), Ernst Cassirer (1955, pp. 209-216), and recently Thomas Pangle (1973, pp. 9, 314) have continued this line of argument. Mark Hulliung (1976, p. 137) and Marshall Berman (1970, pp. 16n, 28n) suggest congruities between themes found in *Persian Letters* and the development of Hegel's account of self-consciousness as the consequence of a struggle for domination. George Kelly (1969, pp. 43-44) suggests a similar congruity between the master-slave doctrine and ideas found in Montesquieu's *Pensées.*

Georg Lukacs (1966, pp. 371, 492) suggested a methodological affinity between Hegel and Montesquieu. In turn, Louis Althusser (1969, p. 49) argued Montesquieu had discovered the concept of a "social totality," and concluded that "Hegel . . . well knew who his master [*maître*] was when he thanked Montesquieu for this discovery." Mercier-Josa (1980, pp. 76-89) traced the influence upon Hegel, and in turn Marx, of Montesquieu's notion of the spirit of a people. Similarly, Paul Thomas (1980, p. 29ff) in his study of Marx discussed the influence on Hegel of Montesquieu's historical psychology.

In addition, Johannes Hoffmeister (1936, p. 464) exclaimed: "Hegel's guide throughout is Montesquieu, not Rousseau." Citing him, H. S. Harris (1972, p. 424n) added, "There can, of course, be no question of the enormous influence of Montesquieu upon Hegel's political and social thought from 1794 onwards." Shlomo Avineri (1972, pp. 4, 6, 185-187) noted the *Philosophy of Right*'s "heavy indebtedness to Montesquieu." Jacques D'Hondt (1968, pp. 154-182) devoted a chapter to Hegel's most obscure reference to Montesquieu. Guy Planty-Bonjour (1974, pp. 7-24) and J.-F. Suter (1971, p. 60n) offered studies on the connection between Montesquieu's "esprit" and Hegel's "Geist." More recently, Charles Taylor (1975, p. 416; 1979, p. 125), George Kelly (1978, pp. 15, 142), and Steven Smith (1982, pp. 9-11) have hinted at Hegel's responsiveness to themes from Montesquieu. In her study of Hegel, Judith Shklar (1976, pp. 157-158) discussed the bearing of the *Spirit of the Laws* on the formation of the *Phenomenology of Spirit.*

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[2] Hegel (1952, pp. 16, 20, 161, 177-178; 1955, III, p. 399; 1956, p. 251; 1964, p. 297; 1966, pp. 39-40, 389; 1971a, p. 156; 1971b, p. 49; 1975a, pp. 128-129; 1975b, pp. 22, 101, 102, 149; 1979, I, pp. 263, 440).

Finally, Michael Oakeshott (1975, p. 251) has argued for the significant affinity of these two authors in terms of their having both theorized a mode of association suitable to "the dominant moral disposition of the inhabitants of modern Europe: the historic disposition to be distinct." He sees the conditions of what he identifies as "civil association" outlined in both Montesquieu's discussion of monarchy and Hegel's discussion (in the *Philosophy of Right*) of the juridical state (Oakeshott, 1975, p. 263; also pp. 245-251, 256-263). These remarks are partially congruent with my argument for an affinity between the two models ("monarchical" and "juridical") suitable to a peculiarly modern mode of association, although I emphasize Hegel's criticism of Montesquieu and consequently his different understanding of both state and mode of association.

Hegel's Debt to Montesquieu

Hegel acknowledges his debt to Montesquieu at the beginning of the *Philosophy of Right.* Paragraph 3 discusses the concept of the whole in which reason "appears" as a particular, and not only as a universal, phenomenon:

> Right acquires a positive element in its content through the particular national character of a people, its stage of historical development, and the whole complex of relations connected with the necessities of nature. . . . Montesquieu proclaimed the true historical view, the genuinely philosophical position, namely that legislation both in general and in its particular provisions is to be treated not as something isolated and abstract, but rather as a subordinate moment in a whole (Hegel, 1952, par. 3, p. 16).

To explain the reasons for Hegel's warm embrace of Montesquieu, we must say something about the nature of the task Hegel set for himself in the *Philosophy of Right.* Hegel accepts Kant's ethical project to depict the state as the rationally purposive setting for the potentially harmonious self-determination of free, equal, and rational beings (Kant, 1963b, pp. 10-30). Hegel's criticism of this project is in part directed against Kant's seemingly unbridgeable dichotomies, which establish reason and freedom as regulative ideals against the actuality of nature and historical circumstance (Kant, 1963a, pp. 11-12, 21). Hegel agrees with Kant's aim—an institutional life characterized as both free and rational—but believes that the two sides of this dichotomy must be more intimately and less hypothetically integrated in order to exhibit the place of rationality and the moral law in human lives.

Hegel subtly shifts, however, the context for the exhibition of freedom and rationality. For Kant, a state's worth lies in its laws reflecting the absolute value of each (rational) being in it. For Hegel, a state's worth follows from the manner in which its juridical structure gives expression to the self-determining "spirit" or mind of its people (Charvet, 1981, pp. 117-135; Hegel, 1952, Preface, pp. 6-7, pars. 4, 33). Through history the spirit of peoples had been variously expressed, but now the occasion has at last arrived for the spirit of a particular people to become the bearer of the Kantian project. The spirit of a people as expressed in the juridical arrangements and understandings of its dispersed members proves to be the only form in which free and rational self-determination can be exhibited. The citizen who understands this relationship to the whole comes to accept that his subjective and individual self-determination is best expressed through conformity with and acceptance of the substantial self-determining collective will as that is constituted by the decisions made through the juridical structure of the state (Hegel, 1952, par. 261-268).

With this conception of the modern state, we are a long way from Montesquieu's defense of "intermediary powers" presided over by a relatively weak although recognizably juridical central state (*Laws* 2.4). Hegel has shifted not only the balance of power but also the locus for the generation of value and worth away from intermediary bodies (which he is yet very insistent have their place) toward the central state institutions.

Nevertheless, to accomplish his Kantian task, Hegel turns to Montesquieu who, he thought, showed him that rational purposiveness could be exhibited in the "particulars" of natural circumstances and historically evolving practices, however much these circumstances and practices, regarded in isolation, seem only capricious and irrational. Hegel seeks "to apprehend and portray the state as something *inherently* rational" (Hegel, 1952, pp. 11-12). The apparently irrational and often freedom-denying circumstances of actual social practices are to be conceptualized along lines suggested by Montesquieu as the vehicles for the ultimate display of reason.

The model Hegel adopts for displaying rationality emerging from apparently irrational circumstances is evidently Montesquieu's monarchical society in which the decaying ethos of the feudal aristocracy—honor—is seen to serve the purposes of integrating a society of individualists. It is, however, just such a society whose decay Hegel contemplates in the *Phenomenology*. Consequently, in the *Philosophy of Right,* the state is not held together through honor, even though the social circumstances recreate the conditions of Montesquieu's monarchy.

The Spirit of a People

The longest of Hegel's early passages in praise of Montesquieu is from his essay on *Natural Law,* where he depicts Montesquieu as a foil to an overly rationalistic Kant on the one side and an overly empirical Locke on the other. Empiricism, Hegel argues, isolates experience when experience should be understood in its relationship to the whole. Empiricists ignore the structured condition of each people that gives them coherence, or an "individuality" as a people. These philosophers "have been shown by Montesquieu" that their reflections on politics are not owed to "experience *a priori,*" but are shaped "wholly and solely by the living individuality of a nation" (Hegel, 1975a, pp. 59-70, 128-129).

The counterpoint to empiricist particularity is idealist universality as articulated by Kant and Fichte. But the idealist conception of the whole is "abstract" both in the sense that it lacks empirical content and in the sense that its point of view is the isolated individual, not men and women considered as members of an ethical order (Hegel, 1975a, pp. 70-76). Hegel intensely disapproves of these features in the thought of his predecessors and rivals. Already he has developed the criticism of Kantian ethics which would show up again in the *Phenomenology* (Hegel, 1975a, pp. 76-80; 1977, pp. 252-261, 365-372). Here, too, Montesquieu is a heuristic example because he did "not merely deduce individual institutions and laws from so-called reason." On the contrary, against both empiricists and idealists, Montesquieu "comprehended both the higher relations [or the universal aims] . . . and the lower specifications [particulars] . . . entirely from the character of the whole and its individuality" (Hegel, 1975a, pp. 128-129).

Hegel's reputation notwithstanding, he offers a partial defense of the empiricist insight, which, along with his damning indictment of "abstraction" in the German idealist tradition suggests as well as anything might why it would be a mistake to read the later *Phenomenology* as a purely conceptual work. It is both conceptual and empirical, both a history of ideas and a history of peoples whose thoughts and experiences are interlocked. Empiricism that claims to be a science is thoroughly condemned, it is true. He ridicules empirical psychology, which attempts to draw up lists "of the capacities found in man." Such endeavors suggest only a "chaos" into which anything can be put and anything can be drawn out. But he praises another kind of empiricism, which although "old and thoroughly inconsistent" is often capable of "a great and pure intuition (Hegel, 1975a, pp. 61-67). Sophocles was such an empiricist of the ethical life of the polis. But empiricism is also the work of theorists. Aristotle is the ancient and Montesquieu is the modern master of this form of thought.

The picture of the social whole which emerges in Hegel's thought is not unfavorable to the empiricist's particulars. The particulars could be said to control the universal in the sense that the universal can only be expressed through its embodiment in the empirical (and particular) world. Hegel found in Montesquieu a system of ordering thoughts about society that demonstrates not only the claim of universal ideals to frame an understanding of particulars, but the reciprocal and parallel claim of the particular as the expressive vehicle of the universal.

This accommodation of idealism to the empirical claim has a moral analogue. In his early writings on Christianity, Hegel inquired into the relationship of the historical Jesus to the Jewish community. Hegel underwent a remarkable change of mind across the space of several years. In the "Positivity of Christianity," the community of ancient Jews is blamed for not being responsive to the high-mindedness of Jesus' intentions. In a subsequent project, "The Spirit of Christianity," instead it is Jesus who is blamed for withholding himself from his community (Hegel, 1971a, pp. 69-74, 205-214, 284-288, 301). In the earlier essay the purity of Jesus' rather Kant-like intentions is portrayed as tragically compromised by the "positive" (meaning historical but also and specifically irrational and authoritarian) circumstances of his culture. By the lights of the later essay, Jesus' isolation from his people, his slighting of their customs for the sake of his good intentions, was tragic. Hegel had shifted ground to argue that only through the medium of "positive" features of a society can abstract good intentions have an effect upon history. Universal reason can win no victories if it is contemptuous of the "positivity," i.e., of the actual historical possessions, of a people, for "all the determinants of ethical relations . . . are determined by the whole." A society or a people have an "individuality." They are "structured" or "constituted" in a certain manner. Hegel identifies Montesquieu as his predecessor in articulating how each particular can become what it is by means of and through the whole.

Montesquieu's Moral Geography

The core idea of Montesquieu's moral geography is succinctly expressed in the *Laws* 8.20:

> It is natural that small states be governed as republics; those of moderate size as monarchies; and that great empires be dominated by despots.

The moral theme underlying this physical criterion is that as states increase in size, the claims that men make upon one another change. In small states, men confront one another as friends or as enemies; in larger states they confront one another as strangers, or sometimes they are largely successful in avoiding the claims of membership altogether. The reasons men increasingly become strangers in their relationships may be more varied than is implied by the imagery of distance. Larger states have more heterogeneous populations. The linkages between various groups and individuals become less immediate. Men have fewer shared presumptions about family background. Finally, they do not have or are not able to conceive of a setting in which each may represent himself in terms comprehensible to all. This is an analysis that looks at larger states from the vantage point of describing what of republican life is absent in them, but this orientation of the theory does not mean Montesquieu is unaware of the advantages of living outside republican polities.

Republics require a people to be more strongly motivated by public friendships and hostilities than they are by any temptation to withdraw into the pursuit of private interest: "One has friends and enemies in the republic; neither one nor the other in monarchies" (Montesquieu, 1951, vol. 1, p. 1063). As a consequence of the more direct presence to one another provided under republican conditions, it is far more important that the citizenry be predisposed to share values than would be the case where the absence of appropriate settings mutes the conflict between values. In a republic, the educative family extends its influence directly over the whole polity, and "it is in republican government that the whole power of education is required. . ." (*Laws* 4.5). The dividing line between republican and monarchical circumstances is the point at which the influence of the family is attenuated: "In monarchies, education begins . . . when children . . . enter the world. This is the school called honor" (*Laws* 4.4).

The differences between a republic and a monarchy are partly a question of government and partly a question of social structure and sentiment. Even when Montesquieu refers to a state's constitution, he often includes aspects we would identify as belonging to the social life of a people. The dominant passions that animate the regime, the "principle," are meant to be a powerful motif in a people's feelings and not simply a style restricted to officers of the government. Because of this ambiguity of reference, it is possible to speak of monarchical social circumstances without pointing to a monarchical state. Monarchical circumstances are those in which humans adopt social arrangements which represent a middle range between communicative closeness and communicative distance. People in this middle range are threatened, on the one hand, with despotic arrangements and are sometimes offered opportunities, on the other hand, to engage in republican experiments, like the English system of parliamentary representation (Montesquieu, 1966b, letter 102, *Laws* 11.6, 19.27). Monarchical society is amenable to a variety of constitutions ranging from those that teeter on the brink of despotism to those that flirt with republicanism. The threat of despotism comes in a variety of forms. Usually Montesquieu's picture of it is connected with the absolutist claims of princes, but sometimes, as in several cautionary remarks about England, he implies that too much republican flirtation under social circumstances more suitable to monarchical order can run the risk of turning in a despotic direction (*Laws* 2.4, 5.11).

An idealist vocabulary makes explicit some of the less available of Montesquieu's theoretical intentions. Thus, human relations in Montesquieu's republican setting could to an idealist observer be characterized by the visibility of others, their direct presence in the schemes of living by which citizens find out who they are. Here one's particular identity is directly implicated in, although even here not completely expressed through, the social presence that the public setting exerts on each. This is "the simple substance of Spirit . . . the immediate certainty of a real ethical situation" (Hegel, 1977, p. 267). Despotic human relations express, on the other hand, the inability to find oneself *in* a publicly available world except as a being who fearfully responds to commands. The republic animated by virtue is, so to speak, the moral equivalent of a philosophical idealist's hope: to see human reason everywhere, to find oneself in the world, as a constituting part of its texture of mutually informing understandings.

And despotism is simply the epistemological denial of this reflection of self in the world.

The most interesting case, once again, is the monarchical society. It puts in question both the citizen's ability to be *present* to others and the citizen's need of that presence. In such a social order the question is raised to what extent we may be what Michael Oakeshott tells us we are, purely self-defining beings whose adventures in self-discovery depend only upon observances of "civility" from others, of whom, unlike the ancient citizen, we do not stand in need (Oakeshott, 1975, pp. 70-78, 234-236, 240-245).

And yet the issue of "need" has not quite disappeared. Two rather general ways of needing one another can be identified. First, each pursues self-discovery partially in responsiveness to the spirit of one's associations, definable as the context and manner of the politically organized availability of others to oneself. Second, each is, however, so subjectively situated as not to have on every occasion the means for knowing of the connections between one's particular role and the commonality of roles in which all may be situated. Each needs that knowledge on occasion, or less democratically, some need it, in order that the whole may possess direction and that the parts may have an understanding of their strategy and of the role in which they want to cast themselves, given the nature of the plot. Despotism is the model for the failure of these two ways of knowing one another; it is always, Montesquieu might be taken as claiming, the implicit threat within any monarchical society.

Hegel's Criticism of Montesquieu

Although both thinkers offer descriptions of the constitution appropriate to modern circumstances, Hegel believed Montesquieu had failed to describe a suitably public life. It was not sufficiently "objective" and lapsed into a "feudal" outlook (Hegel, 1952, p. 178). Curiously, Marx thought Hegel had failed in the same task. Hegel's celebration of the "universal class" was yet another accommodation to particularism (Marx, 1967, pp. 177-190). The two critiques—Marx's critique of Hegel, and Hegel's of Montesquieu—resemble one another. Both critiques focus upon the inability of the predecessor to see how those who participate are likely to distort the universal claims of the state to their private and particular ends.

Did Hegel miss in his own work what he perceived as an error in the work of others? What Hegel learned from Montesquieu inclined him to view politics in a way which, despite specific criticisms of his predecessor, opened him to this sort of challenge from successors like Marx.

For Hegel a modern mass society of detached individualists could create an unhealthy polity. As a corrective, Hegel turns to decentralized corporations or communities as a means of encouraging participation rooted in concrete and particular existence. Like Montesquieu's "intermediary bodies," these try to preserve a sense of place, of local, and possibly of religious, ethnic, and class identity (Hegel, 1952, pars. 250-256, 302, 308). The preservation of such identities is important because, as Hegel suggested in the *Phenomenology,* contemporary societies are more and more constituted by individuals released from every purpose but the pursuit of wealth and power. The search for "abstract" wealth and power estranges people from any contest that could supply them with a sense of membership with others (Hegel, 1977, pp. 313-314). Nevertheless, by redressing these problems with a push toward a more decentralized polity, another problem emerges. It was a problem Hegel attributed to Montesquieu's monarchical order, but not evidently to his own model. Montesquieu's constitution gives too much reign, Hegel believes, to the "subjective" and dissociated prejudice of each; it gives too great a play to particularity and hence undermines a conception of authority to which objective duty could be owed. In the *Philosophy of Right* Hegel claims:

> The fact that Montesquieu discerns 'honor' as the principle of monarchy makes it clear that by monarchy he understands not . . . the type organized into an objective constitution, but only feudal monarchy. . . . This type of constitution rests on privileged persons. . . . The result is that their services are the objects not of duty but only of ideas and opinions (Hegel, 1952, p. 178).

Carving out spheres of partial autonomy, even as a means of avoiding the anomie of a mass society, is to confer privileges that effectively inhibit the creation of a public life to which duty is owed.

Here, of course, is a striking irony. Marx's complaint about Hegel's "universal" class (especially equipped to attend to "objective" duties, Hegel thought), is that its members are too particularized, too concretely embodied to participate on behalf of universal interests. This complaint merely echoes Hegel's views on Montesquieu. Indeed, one may assert that the history of political thought since the eighteenth century has consisted largely in endlessly repeated challenges to predecessors and rivals either that one has gotten to the state but left the citizens behind as shadowy abstractions, or that one has never escaped the presence of privileged citizens who obscure the state and its legitimate purposes altogether.

Thus, the political problem of the modern state has been envisaged by many thinkers as one of finding a sufficiently neutral definition of the uses of state power so that no particular agent may suspect that when he defers to the state he is only obeying the commands of another particular agent (see Ackerman, 1980; Dworkin, 1978). As a result (and not inappropriately), a style of accusatory politics has emerged in which each accuses the other of having captured the public authority to private advantage. Marx's and Hegel's complaints about their predecessors' views are a part of this politics.

Behind an application of impersonal rules, one often finds the rule of persons, as well as of classes, interests, factions, and parties. Each of these specific entities has been at one time or another partially rehabilitated by political theory, but what has often remained untouched is the background belief that an unalterable tension and antagonism must exist between particular agent and general purpose. It is settled belief that the general ideals and universal aims are inevitably tainted by the particular agents of their construction.

In the neutral state, or in one that upholds ideals whose universality stretches to every agent, the main participants in the construction of this form of purity will sooner or later be recognized as the central obstacles to its achievement. This complaint arises again almost by definition: particular actors with general goals are viewed as if they were in contradiction with themselves, for their particularity is understood to be in necessary conflict with, and a source of corruption of, general ideals. The citizen of the neutral state is a divided consciousness who cannot integrate the universal and particular features of his life.

Montesquieu did not choose to search, significantly, for untainted purity in politics. His first book, *Persian Letters,* is a satire on men and leaders who claim to embody universal ideals. Neither Usbek nor the King can succeed. Montesquieu seems to dissent from a tradition whose concern is to define and defend a politics of neutrality. In its place, Montesquieu offers the reader of the *Laws* states in which privilege frankly plays a role. Privilege is necessary to the articulation of the general good. In his monarchical constitution, privileged orders are defended on the grounds that their very resistance to officially defined public purposes forms an essential ingredient in assuring that those official purposes retain their public character (Montesquieu, 1966b, letters 2, 6, 8, 16-18, 155; *Laws* 2.4, 3.5-7, 4.2).

Montesquieu does not say that every form of privileged participation or privileged dissent is justified. Indeed, in a society held together by laws as well as by the cult of honor, whatever sense of distinctiveness, and hence privilege, the latter generates must not be inconsistent with the purposes of a rule-oriented polity.

Communicative Competence and Neutrality

Montesquieu's concerns can be expressed in terms of the contemporary notion of "communicative competence." For Montesquieu, particularity is respected to the extent that it contributes to the "communicative competencies" required on behalf of the universal goals of a polity. Hegel does not reject this criterion for a well-ordered polity. But Hegel, unlike Montesquieu, is more preoccupied with the intellectual inheritance of the social contract tradition. His conceptualization of the state as the result of "wills" striving to establish an order responsive to "freedom" represents his effort to preserve the central intentions of this tradition (although abandoning what he regarded as the contractualist's insensitivity to the Montesquian history of peoples). Contractualist metaphors and assumptions push the theorist in the direction of legitimizing only "neutral" states, i.e., states that preserve the "freedom" of every "will," thus setting up the kind of debate cited above which is the inevitable result of specifying universal goals (of "neutrality") impossible for particular agents to realize (see also Ackerman, 1980; Dworkin, 1978; Riley, 1982, ch. 6).

It may be true that "communicative competence" is a notion wholly compatible with the neutrality or "ideal speech" requirement (Ackerman, 1980; Dworkin, 1978; Habermas, 1979).[3] Then, citizens equally free because not discriminated against or dominated by unneutral institutions will be those most likely to establish relations of communicative competence. Hegel's criticism of Montesquieu suggests an alternative, however. Communicative competence is the criterion for measuring the contributions of different classes of particular agents, whereas neutrality is a separate criterion not readily harmonized with the demand for communicative competence. Montesquieu adopted the former but not the latter criterion, because there is no conceptual reason to suppose that a free and equal citizenry (as established by the demand for a neutral state) will be the only or the best citizenry communicatively competent enough to sustain itself over time. Nevertheless, competence does not create "objective" duties. Only the just state, as measured by "neutrality," can do this. Hegel had to synthesize competence and neutrality.

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[3] This is not the occasion for a close examination of Habermas's ideas, nor of the ideas of the Anglo-American defenders of liberalism. Here I am suggesting an affinity between these ideas (of communicative competence and of neutrality) as a means of exhibiting the significance of Hegel's critique of Montesquieu. For a similar view of Habermas, see Skinner (1982).

Hegel adopts a part of Montesquieu's politics, while believing he can reject its costs. Marx's criticism of Hegel suggests that the costs must be acknowledged. Thus, Hegel's "universal class" is rooted in a particular culture, class, and outlook. This rootedness serves the same purposes that the "family" and "corporation" do. These "intermediary bodies," to use Montesquieu's term, remind men and women of limits, which the pursuit of the "abstract" goals of wealth and power do not, and school and sustain them in becoming communicatively competent. Montesquieu understood that by sponsoring the flourishing of intermediary bodies he was commending a society where power was effectively decentralized and the ordinary conduct of these competing groups would work to the establishment of inequality and the unneutral preference of certain elites over others. Hegel stresses the first, the role of decentralizing institutions, without requiring that the state yield its power to groups that would tend to sanction the second, privileges of one group over those of another. To be sure, Hegel advocated elite rule, but only when it serves the defensible criterion of the social contract theorists (as reformulated by Kant). Marx's criticism, to the extent that it is true, can be taken as a reminder that Hegel never offers his reader good arguments for his presumption that this "particular" universal class would in fact serve these goals. Marx's indictments cannot, by the same token, be taken as a claim that there could ever be a universal class without particular interests.

To review and conclude, it is by looking at the common problem linking Montesquieu and Hegel that we can understand these political dilemmas that question the proper balance between centralizing and decentralizing social practices as well as suggest the uneasy alliance between communicative competence and neutrality. Both theorists believed that societies or "peoples" could be conceptualized as "individuals" whose distinctive identities survive temporal changes. But as societies increased in scale, the sense in which a given people could be so connected as to constitute an "individuality" became more problematic. There would come a point in which the communicative competence so visibly apparent in the small-scale republic would be undermined by the conditions isolating persons from one another. These conditions tend towards despotism. The more interesting case then concerned those societies not visibly united (as in the small-scale republic) and not spiritually and communicatively exhausted (as in a despotism) but hovering between and on occasion flirting with either extreme. But here the agreement between Montesquieu and Hegel ends. The "spirit" of such associations, and the manner of their flirtation with individualistic isolation, on the one hand, and communitarian expressiveness, on the other, turns out for these theorists, as for us, to be matters of unending dispute. Oakeshott's asserted affinity between Montesquieu's and Hegel's description of an appropriate mode of association (expressed as a release from devotion to a common enterprise but in subscription to terms of less restrictive "practice") covers a variety of disagreements over the precise shape of the polity. It leaves the reader with no practical advice, as might be suggested in another way by his (and my) claim that two such differently committed authors could have begun their engagements with politics from such similar premises.

Montesquieu's and Hegel's typologies record the differences made by expanding scale. These are registered in two contrary claims: cohesiveness dissipates, but at the same time the occasions for confrontation diminish. The first predicts instability because it understands decreasing involvement in common projects as a process for dissociating persons from social matrices through which they achieve meaning and a social identity. The second predicts greater potential stability upon the grounds that there is opportunity as well as danger in the process of dissociation: the decreasing need of intense communicativeness as support for the polity means that men and women may possess a "personal" identity partially separable from their social identity. Intensely communicative polities were in effect sensitive barometers of change because every change or withdrawal of energy from the common project seemed to threaten political stability and hence to require the approval of political gatekeepers. Larger societies create spheres of indifference, where, as Montesquieu once put it, even "public crimes" are "more private" (*Laws* 3.5). Hegel echoes these sentiments with his remark that in the sphere called "civil society," i.e., in a country's economic practices, one contemplates "ethical society split into its extremes and lost" (Hegel, 1952, par. 184).

Does then this sphere of indifference promote what I identify as the uneasy alliance between communicative competence and neutrality, or does it mask the subtler facts of domination? Curiously, Hegel's intentions in the *Philosophy of Right* discourage this line of inquiry. Patrick Riley has astutely noted that Kojève and his disciples are wrong in supposing that the key to Hegel's philosophy lies in his description of self-consciousness as constituted by a struggle for recognition between masters and slaves. The *Phenomenology* is structured, true, in accordance with the concept of an evolving consciousness unreleased from this struggle. But the *Philosophy of Right* is premised upon a concept of a rational will for which the problems of this consciousness, i.e., of the enslaved will, are identified as a "comparatively primitive phenomenon" (Hegel, 1952, par. 57; Riley, 1982, pp. 189-190). Riley's observation is a valuable corrective to the Kojèvian tradition, but at the same time it suggests something about the presumption of Hegel's text. The problems of a will enslaved by whatever means to another may be "primitive," but its absence as a informing assumption from the *Philosophy of Right* should be taken as a sign of Hegel's utopian intentions, whatever he may have claimed on behalf of his concept of the will being also the emergent history of modern European nation states. Put another way, to the extent that this text abstracts from the central concerns of the *Phenomenology* to capture the character of enslaved and hence non-autonomous wills, to that extent Hegel is presupposing a society in which none of the subtler forms of domination and coercion analyzed in the *Phenomenology* is present. Hegel explores the statecraft permitted to uncoerced wills, but one should not suppose that this statecraft is possible in a society where Hegel's earlier description of consciousness is still relevant. Here Habermas's preoccupation with the subtler circumstances of domination suggests a return to the *Phenomenology,* even if Habermas's ideal of communicative competence and, implicitly, neutrality speaks more to the traditions established by the *Philosophy of Right.*

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The Relative Influence of European Writers on Late Eighteenth-Century American Political Thought

DONALD S. LUTZ
University of Houston

Drawing upon a comprehensive list of political writings by Americans published between 1760 and 1805, the study uses a citation count drawn from these 916 items as a surrogate measure of the relative influence of European writers upon American political thought during the era. Contrary to the general tendencies in the recent literature, the results here indicate that there was no one European writer, or one tradition of writers, that dominated American political thought. There is evidence for moving beyond the Whig-Enlightenment dichotomy as the basis for textual analysis, and for expanding the set of individual European authors considered to have had an important effect on American thinking. Montesquieu, Blackstone, and Hume are most in need of upgrading in this regard. The patterns of influence apparently varied over the time period from 1760 to 1805, and future research on the relative influence of European thinkers must be more sensitive to this possibility.

The bicentennial of the American founding era has led to renewed interest in the origins and nature of American political thought. One aspect of this interest has been heightened concern with a thorny question that has exercised historians and political scientists for a good number of years—the relative influence of European thinkers on the American founders as they designed their political institutions at the state and national levels.

Shalhope (1972, 1982) has summarized the debate in two historiographical essays. What he calls the "orthodox" view held that John Locke's ideas dominated American political thought until Thomas Jefferson introduced the republican thought of the English Civil War authors during the post-Confederation period. Two writers frequently cited as prominent among the many holding to the orthodox view are Hartz (1955) and Becker (1958).

On the other hand, Robbins (1947), Rossiter (1953), and Adair (1956) were prominent in dissenting from the orthodox view and in pointing out the non-Lockean roots of our political tradition. In her book, *The Eighteenth Century Commonwealthman* (1959), Robbins argued for the importance of the English libertarian heritage to colonial and revolutionary Americans. Men such as Harrington, Milton, Sidney, Neville, Molesworth, and Trenchard and Gordon, she argued, had a central and continuing influence on early American political thought. The quickening interest in the topic on the part of historians produced many important contributions that did not directly support Robbins's work, but the work of others, including Main (1961), Levy (1960), Elkins and McKitrick (1961), Handlin and Handlin (1961), Miller (1961) and Pocock (1965, 1975) fleshed out the importance of English Whig thought.

Just when a new synthesis seemed needed, it was provided by Bailyn (1965, 1967; but see Pocock, 1981). Bailyn identified five major sources from which American colonists drew their political thinking—the writings of classical antiquity, the writings of Enlightenment rationalism, the tradition of English common law, the political and social theories of New England Puritanism (especially covenant theory), and the writers identified by Robbins as being associated with the English Civil War and Commonwealth period. According to Bailyn, this last group, the radical English Whigs, generated the perspective that brought order and synthesis to the other strands of writing, and more than any other source "shaped the mind of the American Revolutionary generation." Wood (1969) wrote a monumental study describing how this synthesis of ideas informed the events surrounding the writing of the constitutions between 1776 and 1789, and a new orthodoxy seemed firmly in place.

However, this new orthodoxy was soon subjected to methodological modifications that have called into question the supposed monolithic character of the republican synthesis stressed by Bailyn and Wood. The post-Bailyn tendency to

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use categories of European writers, sometimes referred to as "traditions," has continued to breed confusion as new categories are "discovered" or assembled. For example, Wills (1978) adds the Scottish Enlightenment as distinct from Bailyn's more general use of Enlightenment. Lundberg and May (1976) correctly note that the term Enlightenment is too broad and hides a great deal of variance in the authors subsumed by the category. They are led to break it into the First Enlightenment (with the likes of Montesquieu, Locke, and Pufendorf), the more radical Second Enlightenment (which includes Voltaire, Diderot, and Helvetius among others), and the Third Enlightenment (typified by Beccaria, Rousseau, Mably, and Raynal). Wills's Scottish Enlightenment becomes the category "Scottish Common Sense" in their schema, and they add the categories Deists and Near-Deists, Devotional and Apologetic, and Romantic. Lundberg and May, on the other hand, do not mention the categories of Common Law, Puritanism, or classical antiquity.

Nor does the use of general categories solve the problem of where many of the major European thinkers belong. Wills places Hume within the Scottish Common Sense tradition, Lundberg and May place him within the Second Enlightenment, and it is not entirely clear where Bailyn places him. Lundberg and May place Hobbes among the English Deists and Near-Deists, whereas Bailyn never clearly places him. Bailyn makes Locke an Enlightenment figure, Lundberg and May place Locke in the First Enlightenment, yet Wills correctly notes that Locke was very often linked by American readers with Algernon Sidney, the great Whig theorist. Without agreement on either a stable set of categories, or the placement of major writers in them, analysis of the relative influence of such "traditions" is problematic.

The prominent alternative of using close textual analysis on major American political writers still generally eschews arguing in terms of broad categories and focuses instead upon specific European writers. The unfortunate tendency here is to identify a single source as dominant. For example, investigations of the texts by Thomas Jefferson have variously concluded that he reflects the dominant influence of the Scottish Enlightenment (Wills, 1979), John Locke (Mahowy, 1979), or the negative influence of Montesquieu (Appleby, 1982). Madison either borrowed many of his ideas from Hume (Adair, 1956-1957), Locke (Devine, 1975), republicanism (Morgan, 1974), or, like Jefferson, wrote in reaction to Montesquieu (Morgan, 1974). We can even still be treated to close textual analysis that completely denies the influence of Whig political ideas on early American political thought (Schmitt & Webking, 1979).

Method of Analysis

If we are to make significant progress toward unravelling this matter of relative influence, it is essential that we move beyond close textual analysis that assumes discipleship on the part of the person whose text is under examination. It will no longer do to examine a text from the American founding era without considering the possibility of multiple influences. This requires more comprehensive identification of those who are candidates for having influenced American political writing. Otherwise we are left with the relatively fruitless debates between those who find one dominant influence as opposed to another, when there is a reasonable probability that the pattern of influence will be multiple and vary from text to text—even in those written by the same author. The purpose of this essay is to advance such a systematic identification of European writers who need to be taken into account.

Contrary to the general tendencies in the recent scholarly literature, the results here indicate that there was no one European writer, or one tradition of thought, that dominated American political reading and writing during the late 1700s. If there was one man read and reacted to by American political writers of all factions during all the stages of the founding era, it was probably not Locke but Montesquieu. As for the relative influence of the various intellectual traditions, the method used here cannot support either the radical English Whigs or the Enlightenment writers as more important—they look about equal in influence. However, there is strong evidence for moving beyond a Whig-Enlightenment dichotomy as the basis for debate on this issue. Debate in the future should include biblical and common law sources as well, just as the number of individual authors deemed important should probably be enlarged and their relative importance reassessed.

Several surrogate measures of influence have already been attempted. One was to examine the educational background of prominent American political writers such as Madison, Hamilton, and Jefferson (Myers, 1981; Wills, 1978). These attempts are limited to a few men, only a few texts, and marred by the assumption that exposure to one or two college instructors was decisive. Another surrogate measure was the examination of the library holdings of a few dozen prominent Americans (Colbourn, 1965). This approach suffered from the assumption that the critical men have been identified and was seriously marred by the assumption that all books in a person's collection were equally valuable to him. A more ambitious approach was to examine all of the booksellers' catalogues from the era, plus the institutional libraries (mainly colleges), library companies, and private libraries (Lundberg & May, 1976). Although a major advance, this expanded effort had the one major problem of not being able to tell us how widely a given volume was actually read. A more useful surrogate measure would be a count of how many times a given volume or author was cited, quoted, or paraphrased, since this would not only help to answer how widely a book was read but also how highly regarded it was.

Approximately ten years ago this author set out with Charles S. Hyneman to read comprehensively the political writings of Americans published between 1760 and 1805. This period was defined as the "founding era" during which the theory and institutions informing the state and national constitutions took final form. Reviewing an estimated 15,000 items, and reading closely some 2,200 items with explicitly political content, we identified and rated those with the most significant and coherent theoretical content. Included were all books, pamphlets, newspaper articles, and monographs printed for public consumption. Excluded was anything that remained private and so did not enter public consciousness, such as letters and notes. Essentially we exhausted all those items reproduced in collections published by historians, the newspapers available in the Library of Congress, the early American imprints held by the Lilly Library at Indiana University, the Huntington Library in San Marino, California, and the Library of Congress. Finally, we examined the two volumes of Shipton and Mooney, *National Index of American Imprints,* for items in the Evans collection of early American imprints on microcard.

The resulting sample has 916 items, which include 3,154 references to 224 different individuals. The sample includes all of the Anti-Federalist pieces identified by Storing (1981) plus 33 more, for a total of 197 Anti-Federalist pieces. It also includes 190 items written by Federalists. Most of these items are identified in Storing (1976); the rest can be found in Hyneman and Lutz (1983), which lists 515 pieces. Althougn not exhaustive, the sample is by far the largest ever assembled, and neither excludes nor emphasizes any point of view. Excluding the proceedings of legislatures and conventions, upon which the sample does not draw, the sample represents approximately one-third of all public political writings longer than 2,000 words published between 1760 and 1805. Also, the distribution of published writings during the era is roughly proportional to the number of citations for each decade.

A citation for purposes of the study is defined as any footnote, direct quote, attributed paraphrasing, or use of a name in exemplifying a concept or position. The primary assumption is that a citation indicates familiarity with the author being cited. Furthermore, it is assumed that the citation is made because potential readers are also likely to be so familiar. Thus, a citation count indirectly provides a sense of the relative frequency with which European authors were read.

Bailyn (1967) has pointed out that such citations and references in the political literature of the founding era often give the appearance of being more window dressing and that often they betray an incomplete understanding of the author or work being cited. Even in those instances where this is true, there must be some familiarity with the cited text on the part of the person writing the pamphlet, as well as some assumed familiarity on the part of potential readers of the pamphlet, if the window dressing is to make any sense. In short, using a citation count has an advantage over close textual analysis in that we can assume familiarity with the text being cited, whether or not the citation is theoretically serious, or whether or not the author has complete command of the text he is citing.

Another advantage is that a citation count need not distinguish between positive and negative citations; to cite another author in order to attack him still shows that the work has been read, and it also shows influence insofar as the cited author's categories of thought are being used. Locke responded negatively to Filmer. Hume responded negatively to Locke. Madison responded positively to certain aspects of Locke and Hume, and negatively to certain aspects of Montesquieu. In each case it is reasonable to assume that a negative citation represents as much familiarity as a positive citation with a cited work, and it is this familiarity we are seeking to establish.

Thus, "influence" is used here in a broad sense. Only close textual analysis can establish the presence of specific ideas in a text, and comparative textual analysis the probable source of the ideas. A weakness of the citation-count method is that it cannot distinguish among citations that represent the borrowing of an idea, the adapting of an idea, the approval of an idea, the opposition to an idea, or an appeal to authority. An advantage of a citation count is that this inability to distinguish the nature of a citation does not matter if all one is trying to do is systematically establish which European writers were consulted and with what frequency. The overview that results provides a good basis for guiding those engaged in close textual analysis to look for influences that might otherwise be missed. A citation count is also a good way of testing the adequacy of the various schemes that have been developed for categorizing European writers by those interested in the relative influence of various intellectual traditions on American political theory.

The basic categorization scheme used is that developed by Bailyn (1967). Although Bailyn's is one of the most prominent categorization schemes and is relatively noncontroversial, there is still room for argument. Where should Locke and Hume be placed? We might view them in one way today, but early American writers emphasized different works by them than we do, and thus viewed them in a way that might be difficult for us to appreciate. To minimize controversy, a citation count both by category and by major individual authors is provided.

The Pattern of Citations for the Entire Founding Era

If we ask what book was most frequently cited by Americans during the founding era, the answer somewhat surprisingly is: the Book of Deuteronomy. From Table 1 we can see that the biblical tradition is most prominent among the citations. Anyone familiar with the literature will know that most of these citations come from sermons reprinted as pamphlets; hundreds of sermons were reprinted during the era, amounting to at least 10% of all pamphlets published. These reprinted sermons accounted for almost three-fourths of the biblical citations, making this nonsermon source of biblical citations roughly as important as the Classical or Common Law categories. Since our concern in this essay is with sorting out the relative influence of European thinkers, the problem of how to count biblical citations is not important. It is relevant, nonetheless, to note the prominence of biblical sources for American political thought, since it was highly influential in our political tradition, and is not always given the attention it deserves (Lutz, 1980).

References to writers identified with the European Enlightenment are fairly constant throughout the 45-year founding era, but the mix of writers within this category changes significantly over the years. One major conclusion suggested by this study is that the relative prominence of a writer usually varies over time, and when discussing relative influence, we should, for example, distinguish the Revolutionary era from the era surrounding the writing of the United States Constitution.

For example, Montesquieu and Locke are very prominent during the 1760s, when the percentage of Enlightenment citations is highest. Together they account for over 60% of all references to Enlightenment thinkers. During the 1770s these two account for over 75% of all references to Enlightenment thinkers. However, the references to the two are structured in an interesting manner. References to Locke in the 1770s are found heavily in pieces justifying the break with England, whereas Montesquieu is cited heavily in pieces dealing with constitutional design. As the writing of state and national constitutions continues in the 1780s, Montesquieu increases in importance to the point where he accounts for almost 60% of all Enlightenment references. Meanwhile, Locke's rate of citation falls off drastically, never to return to prominence. After the writing of the national Constitution, references to Montesquieu also fall off and are limited primarily to pieces related to the writing of state constitutions during the 1790s. This pattern should not surprise us at all upon reflection. Locke is profound when it comes to the bases for establishing a government and for opposing tyranny, but has little to say about institutional design. Therefore his influence most properly lies in justifying the revolution and the right of Americans to write their own constitu-

Table 1. Distribution of Citations by Decade (%)

	1760s	1770s	1780s	1790s	1800-05	% of Total Number
Bible	24	44	34	29	38	34
Enlightenment	32	18	24	21	18	22*
Whig	10	20	19	17	15	18
Common Law	12	4	9	14	20	11
Classical	8	11	10	11	2	9
Peers	6	2	3	6	5	4
Other	8	1	1	2	2	2
	100%	100%	100%	100%	100%	100%
N	216	544	1306	674	414	3154

*If we break Bailyn's Enlightenment category into the three sub-categories described by Lundberg and May, the results are not significantly altered. The "First Enlightenment," dominated by Montesquieu, Locke, and Pufendorf, comprises 16% of all citations. The more radical writers of the "Second Enlightenment," men like Voltaire, Diderot, and Helvetius, garner 2% of the citations. The "Third Enlightenment," typified by Beccaria, Rousseau, Mably, and Raynal, receives 4% of the citations, to bring the total back to the 22% listed here.

Table 2. Most Cited Thinkers by Decade[a]

	1760s	1770s	1780s	1790s	1800-05	% of Total Number
Montesquieu	8	7	14	4	1	8.3
Blackstone	1	3	7	11	15	7.9
Locke	11	7	1	1	1	2.9
Hume	1	1	1	6	5	2.7
Plutarch	1	3	1	2	0	1.5
Beccaria	0	1	3	0	0	1.5
Trenchard & Gordon	1	1	3	0	0	1.4
Delolme	0	0	3	1	0	1.4
Pufendorf	4	0	1	0	5	1.3
Coke	5	0	1	2	4	1.3
Cicero	1	1	1	2	1	1.2
Hobbes	0	1	1	0	0	1.0
	33	25	37	29	32	32.4
Others	67	75	63	71	68	67.6
	100%	100%	100%	100%	100%	100.0%
N	216	544	1306	674	414	3154

[a] This table is limited to those who were cited at least 32 times, which is 1% of the total of 3,154 citations. The extra decimal point in the last column is to allow more precise recovery of the number of citations over the era, whereas all other percentages are rounded off to the nearest whole number to ease the viewing of the table. The use of 0% indicates fewer than .5% of the citations for a given decade.

tions rather than in the *design* of any constitution, state or national. Locke's influence has been exaggerated in the latter regard, and finding him hidden in passages of the U.S. Constitution is an exercise that requires more evidence than has hitherto ever been provided.

Montesquieu's prominence during the period of constitution writing is supplemented by the relative prominence of two other Enlightenment writers—Beccaria and deLolme (usually written Delolme). It is also during this period of constitution writing that a host of English Whig writers becomes prominent. "Cato" (Trenchard and Gordon), Hoadley, Bolingbroke, Price, Burgh, Milton, Rollin, Molesworth, Priestly, Macaulay, Sidney, Somers, Harrington, and Rapin were most heavily cited during the late 1770s and the 1780s. They are joined by other Enlightenment writers including Robertson, Grotius, Rousseau, Pope, Raynal, Mably, Burlamaqui, and Vattel. All in all, during the period of constitution writing the Enlightenment and Whig authors were cited about equally as a group, with the references to the Whigs spread over about three times as many authors. There is no Whig author to compare with Montesquieu for sheer volume or dominance of the category. Indeed, Montesquieu is almost without peer during the founding era for prominence, except for Blackstone.

Blackstone is the second most prominent secular writer during the founding era. He is cited well over two and a half times as often as Locke. Whereas Locke's pattern is toward relative prominence early during the founding era, falling off after the 1770s, Blackstone's pattern is that of increasing frequency of citation after the 1770s to achieve prominence late in the founding era. Hume follows a similar pattern. Both Blackstone and Hume are strong on governmental process and the operation and interaction of institutions. There is a certain logic, then, in their becoming prominent during the portion of the founding era when the operation, adjustment, and evolution of political institutions becomes of greater concern than their design. This is not to say that the matters of foundation and institutional design are never discussed by Blackstone and Hume, since they are. And the two are cited in this regard during the early years of the founding era. These two also become vehicles for extending Locke's visibility indirectly. Blackstone himself cites Locke a number of times, and certain of his institutional and procedural concepts seem to be grounded in Locke insofar as they are congruent with Locke's principles, or logically implied by Locke. Hume, on the other hand, was one of Locke's most severe critics. To a certain extent his work is in opposition to Locke and can be viewed as running contrary to some of the implications contained in Locke's writing.

There does not seem to be at this time any basis for explaining the pattern followed by citations to

Table 3. Ordering of Most Cited Thinkers, 1760-1805[a]

	(%)		(%)
1. Montesquieu	8.3	19. Shakespeare	.8
2. Blackstone	7.9	20. Livy	.8
3. Locke	2.9	21. Pope	.7
4. Hume	2.7	22. Milton	.7
5. Plutarch	1.5	23. Tacitus	.6
6. Beccaria	1.5	24. Coxe	.6
7. Trenchard & Gordon	1.4	25. Plato	.5
8. Delolme	1.4	26. Abbé Raynal	.5
9. Pufendorf	1.3	27. Mably	.5
10. Coke	1.3	28. Machiavelli	.5
11. Cicero	1.2	29. Vattell	.5
12. Hobbes	1.0	30. Petyt	.5
13. Robertson	.9	31. Voltaire	.5
14. Grotius	.9	32. Robison	.5
15. Rousseau	.9	33. Sidney	.5
16. Bolingbroke	.9	34. Somers	.5
17. Bacon	.8	35. Harrington	.5
18. Price	.8	36. Rapin	.5

[a] Includes all thinkers cited at least sixteen times (.5% out of the total number of 3,154 citations). These 36 names account for 47.8% of all citations.

Pufendorf and Coke during the era. The classics are cited rather consistently, although there is a sharp drop toward the end of the founding era. The category "Peers" is a bit of a misnomer; about a fifth of these citations concern Americans dead by the time they were cited, although all but a few wrote during the 1700s. The term "Peers" is used instead of "Americans" because during the 1760s almost all of the citations in this category are to members of the English Parliament or to articles and essays written by men in England. After 1770, about a third of the references are to documents such as a state constitution or a resolution passed by a state legislature. There is not one of prominence among those cited, in the sense that no one is cited very often. Benjamin Franklin gets a few references, as do Thomas Paine and Thomas Jefferson, but it is a very diverse category.

The Pattern of Citations from 1787 to 1788

Tables 4 and 5 illustrate the pattern of citations surrounding the debate on the U.S. Constitution. The items from which the citations for these two tables are drawn come close to exhausting the literature written by both sides. The Bible's prominence disappears, which is not surprising since the debate centered upon specific institutions about which the Bible had little to say. The Anti-Federalists do drag it in with respect to basic principles of government, but the Federalists' inclination to Enlightenment rationalism is most evident here in their failure to consider the Bible

Table 4. Distribution of Citations: Federalist Versus Antifederalist

	Federalist (%)	Antifederalist (%)	Total for 1780s (%)
Bible	0	9	34
Enlightenment	34	38	24
Whig	23	29	19
Common Law	8	12	9
Classical	33	9	10
Peers	1	2	3
Other	1	1	1
	100	100	100
N	164	364	1306

Table 5. Twenty Most Cited Thinkers: Federalist Versus Antifederalist

	Federalist (%)	Antifederalist (%)	Total for 1780s (%)
Montesquieu	29	25	14
Blackstone	7	9	7
Locke	0	3	1
Hume	3	1	1
Plutarch	7	0	1
Beccaria	0	4	3
Trenchard & Gordon	2	2	3
Delolme	0	6	3
Pufendorf	0	1	1
Coke	0	1	1
Cicero	0	1	1
Robertson	0	0	1
Licurgus	6	1	1
Mably	7	2	2
Grotius	5	0	1
Temple	5	1	1
Price	0	2	1
Addison	0	2	.5
Vattel	0	1	.5
Sidney	1	0	.5
	72	62	44.5
Other	28	38	55.5
	100	100	100.0
N	164	364	1306

relevant. Surprisingly, both sides use Enlightenment and Whig authors in about the same proportion.

Montesquieu is almost twice as prominent during the debate over the national Constitution as he is for the decade as a whole, and three to three-and-a-half times as prominent as he is for the entire era. Grotius and Mably are the only other Enlightenment figures mentioned prominently by the Federalists, whereas the Anti-Federalists use Delolme, Beccaria, Mably, Price, Vattel, Pufendorf, and Locke to their advantage. Among Whig writers, the Federalists favor Trenchard and Gordon, Temple, and Sidney, whereas the Anti-Federalists favor Price, Addison, and Trenchard and Gordon about equally. Despite these differences, the most interesting finding is how similar the Federalists and Anti-Federalists are in their citation patterns. Not only do we *not* find the Federalists inclined toward Enlightenment writers and the Anti-Federalists away from them, the Federalists sometimes cite Enlightenment writers while attacking them. For example, there is a lot of arguing against Montesquieu's dictum that republics must be small and homogeneous if they are to survive, while the Anti-Federalists cite Montesquieu with approval on this point.

The debate surrounding the adoption of the Constitution was fought out mainly in the context of Montesquieu, Blackstone, the English Whigs, and major writers of the Enlightenment. The Federalists also have a third of their references to classical thinkers, while the Anti-Federalists have an average level of such citations. The classical thinkers provided exemplars of practices, leaders, and behavior—often negative ones—but generally were not drawn upon for concepts, terms, and institutional analyses that are most appropriate to such a debate. Hence the Federalists cited most heavily Plutarch, not Aristotle, Plato, or Cicero.

Conclusions

There is hardly an historian or political scientist working in this area who would be surprised by the presence of Montesquieu in the citations, but his prominence turns out to be so great, and his appeal so wide across all factions, it is surprising that so little beyond Spurlin (1940) has been written about him in the American context. It is time to consider as carefully the influence of Montesquieu on specific political texts as we have sought the influence of Locke.

The prominence of Blackstone would come as a surprise to many, and he is the prime candidate for the writer most likely to be left out in any list of influential European thinkers. His work is not readily available in inexpensive form, but like Montesquieu he was cited frequently by all sides. A trenchant reference to Blackstone could quickly end an argument. Such a respected writer deserves a much closer look by those studying American political thought.

There is good reason to treat Locke's influence with greater care. Even though the motto *Locke et praeterea nihil* as it applies to eighteenth-century American political thought has been thoroughly discredited by historians, there is probably still a tendency to overestimate his importance. Furthermore, the Locke we read today was not the Locke generally read then. Today we are likely to read his *Second Treatise,* whereas during the founding era Americans were much more likely to read *An Essay Concerning Human Understanding.* References to Locke's *Second Treatise* often indicate a relative lack of understanding—as if they are relying more upon general hearsay that upon a direct reading. Indeed, Lundberg and May (1976) demonstrate that the two treatises had only about one-third the availability of *An Essay Concerning Human Understanding* from the libraries and booksellers of the era. The availability of the *Second Treatise* about matches that of Addison's *Evidences of the Christian Religion,* Pope's *Essay on Man,* or Wollaston's *The Religion of Nature Delineated.* There is no question that Locke was important for American political thought, but he needs to be placed in context and his influence more carefully assessed.

There is evidence that Hume should be considered about equal in influence to Locke, and that Hume is more important for theory surrounding the writing of constitutions when it comes to content. The Hume read then is also not the Hume we are likely to read today. Citations to his work come overwhelmingly from *The History of England* rather than from his *Essays and Treatises on Several Subjects* or his *Dialogues Concerning Natural Religion.* There is no doubt that his Essays and Treatises was highly influential on some Federalist minds, however, such as Madison's and Hamilton's, and the availability of this work precisely matched that of Locke's two treatises. Hume deserves a much more careful look. Beyond this, we need to pay more attention to a whole host of Enlightenment, Whig, and Common Law theorists. The 36 names listed in Table 3 are all possible candidates.

It is interesting that the writers of the so-called Scottish Enlightenment—Francis Hutcheson, David Hume, Adam Smith, Thomas Reid, and Adam Ferguson—are, with only one exception, not very prominent among the citations. In the case of Hume, we may today read him as a member of such a category, but Bailyn and others are probably correct when they say that Americans during the founding era often saw him as an exponent of Whig republicanism, or else as a covert Tory.

Finally, the patterns of influence apparently varied over time. The current literature is not sensitive to this possibility, and too often a close textual analysis of one or two documents written in, say, 1776 or 1788, allows the establishment of one European author's influence to stand for the entire era. We need to consider the extent to which the debate surrounding the adoption of the U.S. Constitution reflected different patterns of influence than did the debates surrounding the writing and adoption of the state constitutions, or the Revolutionary writing surrounding the Declaration of Independence. Examining more carefully the differences and similarities in such patterns should lead us to a firmer understanding of the intellectual divisions within American political thought at the time, divisions that increasingly appear to be more complex than is usually credited in recent political science literature.

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Communications

Updating Political Periods and Political Participation

TO THE EDITOR:

We demonstrated in a recent article (Beck & Jennings, 1979) that, contrary to conventional wisdom, the relationships between age and participation and between ideology and participation varied considerably over time. Part of the supporting data used in our analysis came from the election studies series collected by the SRC/CPS of the University of Michigan. In this note we update this analysis to include the results from the 1980 election study.

We turn first to the association between age and campaign activities. The campaign participation index was constructed in the same fashion as previously and is based on the sum of five different types of activities. A comparison with previous years (Figure 1) demonstrates three significant points. First, our contention that the age-participation nexus is a variable one receives further confirmation. The 1980 results depart considerably from the 1976 and 1972 patterns, especially with respect to the relative ordering of the three youngest cohorts and the distances separating them. The overall 1980 results are much more in accord with those from 1968 and before.

A second, related point is that the 1980 results highlight even more the exceptional character of the 1972 campaign. Whereas the participation rate for the youngest cohort in 1980 trails that of the second youngest—as was the case for all of the pre-1972 distributions—the young cohort in 1972 had an extraordinarily high activity rate. This is highlighted by its slump in 1976 to near-normal levels.

A third interesting facet of the 1980 results is the upsurge in participation by the aged. Their absolute participation is fluid over time, although in all years they have the lowest absolute rate. But the nadir of their exertions came in 1972, concomitant with the zenith achieved by the very young. The forces that attracted and mobilized the young may have been the same ones that repelled and immobilized the old. By 1976 the elderly had regained their lost ground, moving to the absolute participation levels they attained in 1956 and 1964. The 1980 rate for the old is exceeded only by the level reached in 1960. Moreover, although the national average in 1980 fell below that of 1972 and barely exceeded that of 1976, the average gain for the elderly was approximately 50%.

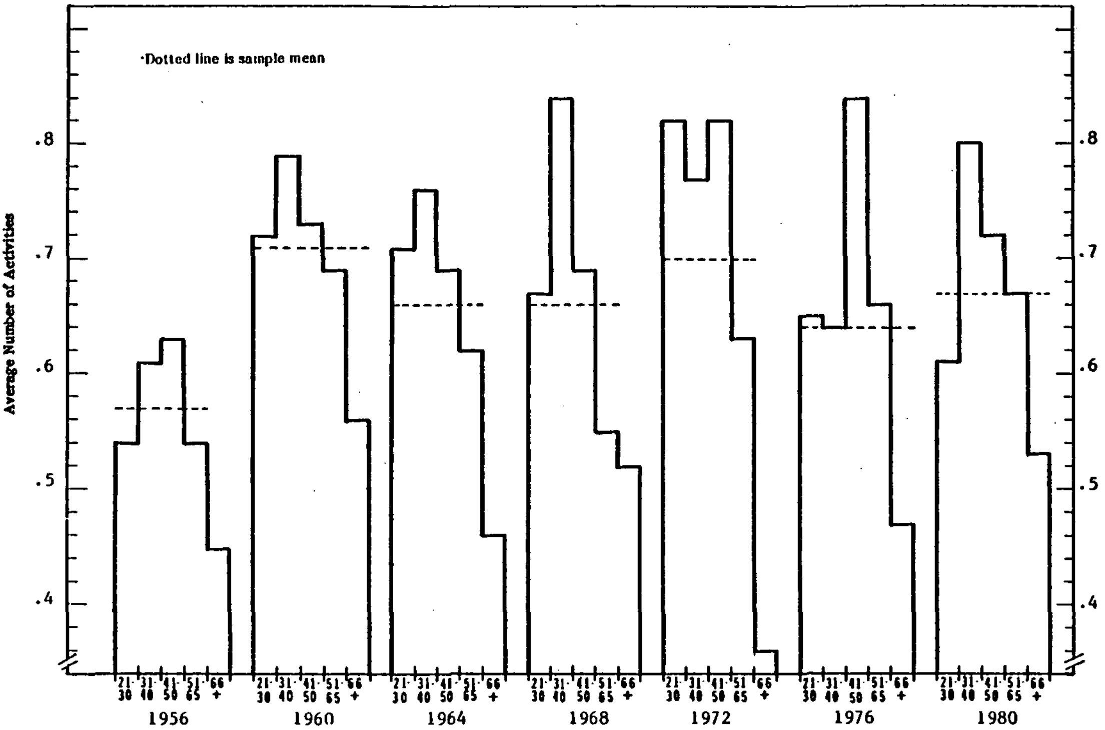
It is tempting to attribute the gain among the old to a "Reagan effect." The presence of an aged, attractive candidate in 1980 could have supplied an extra incentive for the older voters. The fact that the elderly gave a disproportionate share of their vote to Reagan compared with other cohorts and with the 1976 GOP vote is at the least suggestive. In addition, whereas 67% of the aged fell on the conservative side of our ideology index (see below), the same was true of only 46% of the 21-to-30-year-olds. Finally, it is probably no coincidence that the 1980 activity level of the youngest cohort was their lowest in absolute terms since 1956—a year of abnormally low activity overall. Inferentially, the presence of candidates with personal qualities and campaign appeals to different segments of the age structure appears to affect their campaign mobilization.

The second argument advanced in our earlier report was that the linkage between political ideology and campaign activity is also variable. Before 1968, conservatives campaigned more intensively than did liberals, thus giving rise to the conventional wisdom about the conservative bias of participation in America. Beginning in 1968, however, the old pattern was disturbed. By 1972 ideological liberals outcampaigned conservatives by far. To assess the relationship in 1980 we recreated the liberal-conservative issue index used for previous years, although the mix of issues is somewhat different from that used in 1976 (Beck & Jennings, 1979, pp. 745-746).

As Figure 2 shows, strong liberals continued to be the most active ideological stratum, increasing their advantage over conservatives from the slim lead held in 1976, although falling far short of their spectacular lead in 1972. The 1980 configuration confirms our argument that the ideology-campaign activity link is not invariant and that the presumed conservative bias was a function of the particular nature of the 1956-1964 era.

However, we might have expected somewhat different outcomes in accordance with our secondary argument about the connection between the ideological fervor of the candidates' campaigns and the participation levels of the candidates' supporters. On its surface, the 1980 campaign appears to have been one in which the "right pole" of the participation opportunity structure was the more highly charged. Reagan was clearly the more

Figure 1. Campaign Activity by Age, 1956-1980



Source: Presidential Election Series, Center for Political Studies, University of Michigan.

glamorous and inspirational candidate (Kinder & Abelson, 1981). The excitement of the campaign lay in his camp rather than in the defensive and often dispirited Carter camp. Thus we might have expected to see an upsurge in the activity levels of conservatives and perhaps a diminution on the part of liberals. All the more so because extra-party organizations on the right claimed to be mobilizing great hordes of heretofore inactive citizens.

Perhaps the special brand of conservatism espoused in 1980 needs to be considered. A peculiar feature of the 1980 campaign was the outspoken efforts on the part of such groups as the Moral Majority and the National Conservative Political Action Committee to mobilize conservative voters. Morality was often stressed, as in the instances of the abortion, welfare, and defense (anti-Russian) issues. We concentrate here on the possibility that the religious right was exceptionally successful in activating citizens with conservative, fundamentalist religious beliefs. Two questions in the 1980 interview schedule dealt directly with religious fundamentalism: 1) whether one has had a "born-again" experience; and 2) whether one believes in the inerrancy of the Bible.[1] In the analysis the responses were scored, as appropriate, from non-fundamentalist to fundamentalist in direction.

If the claims of the religious right are correct that there was a special sort of conservative upsurge in 1980, then the more fundamentalist individuals should have been more active than the less fundamentalist, other things being equal. The latter condition was met by examining the relationship between fundamentalism and campaign activity according to educational level among blacks and whites separately. Education was used as a control because it is the best and most reliably measured social predictor of participation and because it is also related (inversely) to fundamentalism. Race was selected because it is related to fundamentalism (blacks higher), and because the corollaries of participation are somewhat different for blacks and whites. Campaign activity was gauged by the standard activity index, which excludes voting; because of the emphasis placed on turnout by the extra-party groups, vote turnout was used as a second indicator.

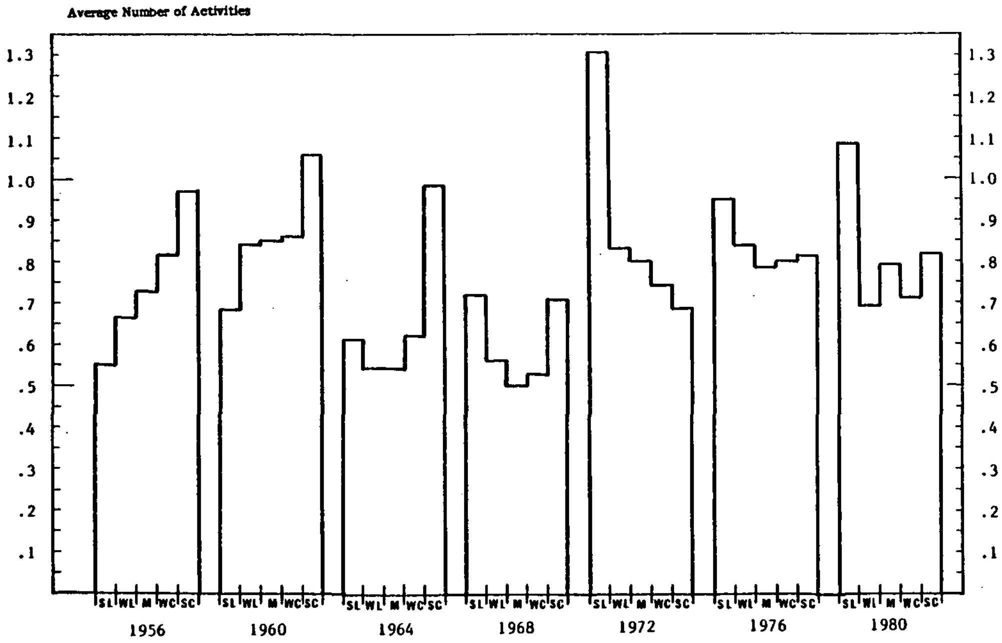
---

[1] These are questions U8 and U9 in the postelection survey.

Altogether there are 24 comparisons to be made —two indicators of political activity × two measures of fundamentalism × three educational categories × two races. In only one of these comparisons did fundamentalism have a statistically significant effect on activity (at the .05 level).[2] Paradoxically, fundamentalism tended to have a more positive impact, modest though it was, on blacks than on whites. Given the overwhelming tendency of blacks to support the usually more liberal Democratic candidates, the higher participation rates inspired by appeals to their religious fundamentalism can scarcely have been considered a blessing by the Moral Majority. Based only on our examination of the 1980 results, it is difficult to argue that the religious right was exceptionally activated in 1980.

In sum, our primary contention about the variable nature of the relationships between age and participation and ideology and participation is supported by the 1980 results. Our secondary contention that differential mobilization is based on the seeming discrepancies in ideological fervor offered by presidential contenders is not sup-

Figure 2. Campaign Activity by Political Ideology, 1956-1980



Source: Presidential Election Series, Center for Political Studies, University of Michigan.

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[2] Among blacks with a high school education, 76% of those who viewed the Bible as "God's word and all it says is true" reported voting in the election as compared with 54% in the next category.

ported. Based on the analysis of the *direction* of the 1980 vote (Markus, 1982), it seems possible that candidate evaluations may have played a key role in mobilization.

PAUL ALLEN BECK

The Florida State University

M. KENT JENNINGS

The University of California, Santa Barbara

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Comment on Iyengar, Peters, and Kinder (Vol. 76, December 1982, pp. 848-858)

TO THE EDITOR:

I applaud the intent of Shanto Iyengar, Mark D. Peters, and Donald R. Kinder's December 1982 article, "Experimental Demonstrations of the 'Not-so-Minimal' Consequences of Television News Programs." I agree that political communication research has too long neglected the unique ability of experimental methods to isolate and probe specific hypotheses about behavior. Nevertheless, conclusions reached in controlled experimental settings should be extrapolated to the world outside the laboratory only with the greatest caution. I fear that Iyengar and his colleagues may be interpreting their results with too much enthusiasm.

Iyengar, Peters, and Kinder report that subjects who viewed four network newscasts reedited to "provide sustained coverage of a certain national problem" were more likely than unexposed subjects to identify the problem as an important national issue (1982, p. 850). These findings, they proclaim, "decisively sustain Lippmann's suspicion that media provide compelling descriptions of a public world that people cannot directly experience. We have shown that . . . television news programs profoundly affect which problems viewers take seriously" (p. 855).

The design they use to come to these conclusions is elegant in its simplicity. Subjects visited their laboratory on six consecutive days. On the first and sixth days, the subjects completed questionnaires measuring various political attitudes. On the days in between, they watched the modified network newscasts—basically the preceding day's broadcast, with the addition of one or more stories from the Vanderbilt Television News Archive emphasizing a particular problem. The investigators report the substantial differences between attitudes on day 1 and day 6 as effects of viewing the altered news programs.

These experiments are forceful evidence that television *can* significantly influence public opinion. But do they "decisively sustain" a "profound" effect outside the laboratory? I'm not convinced.

The problem with this elegant simplicity is that it is too simple. The Iyengar group's subjects received a much more powerful stimulus than the typical viewer at home—a stimulus with unusually concentrated viewing time and focused subject matter. Iyengar and his colleagues have shown that hitting the public over the head with a two-by-four changes attitudes, at least in the short term. They haven't shown that significant, enduring change occurs when the catalyst is as diffuse as it often really is.

In the first place, most people don't catch the network news faithfully night after night, as these subjects did. Only 38% of the 1980 National Election Study's traditional time-series sample said they watched network newscasts every evening (Table 1), and these responders were disproportionately older and black. Six out of 10 Americans do watch at least three network news programs weekly, but 20% see the network news less than once a week.

Second, few real-life stories receive as much attention as these investigators devoted to their experimental stimulus—three to six minutes four nights running, including at least two pieces four minutes or longer (about one-sixth of an average

Table 1. Frequency of Network News Viewing

How often do you watch the national network news on TV?	
Every evening	38%
Three or four times a week	24
Once or twice a week	18
Less often or never	20
	100%
N	1363
Missing cases	45
Sample size	1408

Source: 1980 National Election Study (C3C3PO).

network newscast, minus commercials). It is worth noting that Iyengar, Peters, and Kinder apparently had to ransack several months of broadcasts to collect the segments they telescoped into four days.

With such extraordinary parameters, it is possible that Iyengar and his colleagues are talking about exceptions, not the rule of how television news affects public opinion. Consequently, I feel uncomfortable projecting their findings beyond the very short term. One wonders if their results might have been quite different if the viewings had been more widely dispersed in time and if attitude change hadn't been measured so soon after the final session.

Only the lunatic fringe of believers in "minimal consequences" ever argued that the media have no impact at all on what the public thinks about; even Lazarsfeld and Merton recognized that the media can confer status on (read "add to the agenda") individuals and ideas. The issue, it seems to me, is not whether or not television news contributes to the political agenda, but under what conditions it does so. Iyengar, Peters, and Kinder have identified one way we can work to answer that question, but haven't supplied an answer that we should take as definitive—at least not yet.

DAVID BLOMQUIST

CBS Inc.[1]

Reference

Iyengar, S., Peters, M. D., & Kinder, D. R. Experimental demonstrations of the "not-so-minimal" consequences of television news programs. *American Political Science Review*, 1982, *76*, 848-858.

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[1] The views expressed here are solely my own.

Comment on Russett
(Vol. 76, December 1982, pp. 767-777)

TO THE EDITOR:

Bruce Russett's "Defense Expenditures and National Well-Being" examined the effect that changes in military spending have had on federal expenditures for health and education. He "could find no regular pattern of trade-offs in the data for the last four decades of American history" (1982, pp. 775-776). Russett's conclusion is based solely on the hypothesized statement of a specific relationship between military expenditures and expenditures for health and education. Why does the perception of trade-offs exist, given his persuasive findings?

Russett focuses on federal outlays. However, the political process directs relatively little attention to outlays per se; the bulk of political capital is invested in the budgetary process, which is in essence a conflict between Congress' and the president's budget priorities. Ensuing outlays more often than not represent compromises (trade-offs) between the president and Congress.

Therefore, to examine merely the endproduct of the debate, federal outlays, may obscure real trade-offs between Congress and the executive. I suggest that the trade-offs occur during the budgetary process where, given the constraint of limited available resources, the president proposes spending levels, based on his policy priorities, and Congress adjusts the numbers based on their priorities. Republican presidents seek to enlarge defense spending at the expense of social programs; Democratic presidents attempt to increase social spending at the expense of military outlays. Congress, in the absence of *drastic* changes in its political complexion (both houses majority democratic, 1955-1980) will attempt to maintain existing programs and federal levels. Therefore, Republican presidents are likely to be continually frustrated in their defense budget requests, getting less than asked for, whereas Democratic presidents are likely to lose on social spending. Conversely, Congress is likely to appropriate more money for social programs than republican presidents request and higher levels of defense spending than Democrats want. It is this pattern of adjustment-readjustment in the budgetary process that leads to the general perception that to increase social spending, defense outlays must be reduced, and vice versa. Some additional data examined here offer an alternative perspective on the trade-off question.

Table 1 shows the relationship between the president's budget estimates of expenditures and the subsequent federal outlays for both military spending and HEW expenditures for the federal fiscal years 1954-1980. Estimates represent the difference between what the president hopes to get (estimates) and what he actually does get (outlays), expressed as a percentage of outlays. Negative numbers indicate that a president received more money than he asked for; positive numbers mean less money than estimated.

For 19 of the 27 years in question, defense and HEW relative estimates had opposite signs; when a president underestimated defense expenditures, he overestimated HEW expenditures, and vice versa. Thus, slightly more than 70% of the time, the hypothesized trade-off between military and civilian expenditures occurs, not in the actual expenditures, but in the relationship between the estimates and the expenditures. Moreover, from 1973 to 1979 a consistent pattern of overestimat-

Table 1. Presidential Budget Estimate Errors[a]

Fiscal Year	President	Defense	Social Security, Health, Education, Welfare
1954	Eisenhower	−0.4	14.7
1955		9.1	−29.0
1956		−0.9	−16.9
1957		−0.4	0.7
1958		−1.2	2.6
1959		−1.3	−17.6
1960		0.4	−6.5
1961		4.0	7.7
1962	Kennedy	−7.3	5.8
1963		−0.2	6.5
1964		2.2	1.8
1965	Johnson	7.6	−1.7
1966		−10.6	3.3
1967		−12.4	−2.0
1968		−4.6	7.1
1969		−1.7	4.9
1970	Nixon	1.5	−3.2
1971		−5.3	−6.9
1972		−1.0	−6.5
1973		3.0	−11.1
1974		3.2	−2.6
1975	Ford	1.3	−7.3
1976		4.4	−8.8
1977		3.7	−2.4
1978	Carter	6.7	−1.4
1979		0.1	−0.1
1980		−7.5	−7.4

[a] Error = (estimate−actual)/(actual).

ing defense expenditures and underestimating civilian expenditures occurs. Given the general lack of historical memory in American politics, this seven-year run may have provided the basis for the Reagan administration's claims that trade-offs in actual expenditures were required.

Table 2 gives us a second perspective on the question of trade-offs by showing the average direction of the presidential budget estimate errors by political party. Only the second, third, and fourth years of presidency are examined. (The first year often represents a transition year that might mask underlying relationships.) The second fiscal year of a presidency is the first year in which the executive estimates fully reflect the current president's priorities. It is the year in which the largest differences between Congress and the presidency are likely to be seen as presidents

Table 2. Average Direction of Presidential Budget Estimate Errors by Party (1954-1980)

	Defense	Social Security, Health, Education, Welfare
Second Year		
Democrats	−11.5	4.9
Republicans	1.3	−15.2
Third Year		
Democrats	−1.4	1.7
Republicans	0.7	−9.7
Fourth Year		
Democrats	−0.8	9.9
Republicans	0.6	−1.3

attempt, but largely fail, to redirect national priorities. In subsequent years, presidents tend to moderate their positions somewhat, creating slightly more convergent behavior. The results in Table 2 lend support to this perspective. Republican presidents have had a tendency to overestimate defense spending and underestimate civilian spending; the Democratic presidents have on average done just the opposite. It appears the executive, regardless of party, perceives that attempts to enhance one type of spending are made at the expense of the other. Which type of spending is underestimated depends on the party affiliation of the president, which I presume, to some extent reflects ideology.

Why does the perception of trade-offs exist? Presidents in their estimates of expenditures have fairly consistently overestimated the need for one type of spending and underestimated the need for the other. Congress, maintaining an incremental approach, has adjusted these priorities, acting as a moderating force. I have only dealt here with relative directions of change which in the strictest sense is what trade-offs concern. Trade-off magnitudes vary considerably and may be influenced by many of the factors Russett suggests. Although the precise nature of this relationship has yet to be fully delineated (and subsequent models examined) this, in part, may account for the widely held view that to increase social spending, reductions in defense spending must occur.

LANCE BROUTHERS

University of Arkansas at Little Rock

Comment on Searing
(Vol. 76, June 1982, pp. 239-258)

TO THE EDITOR:

Although Donald D. Searing reports an excellent and significant study of "British politicians' thinking about constitutional topics" (p. 239), the article stresses the weakest findings. As Searing says, the unwritten British Constitution consists of "rules of the game," such as the supremacy of Parliament. Searing investigated support among members of Parliament for the "rules of the game," for example, agreement that "there should be no limitations on Parliament's power to make or unmake any law whatsoever."

As Searing briefly states, the major findings are that there was a consensus on "rules of the game," that no group consistently opposed constitutional norms, and that absence of support was largely unpatterned (p. 243). Such solid empirical support for our prior expectations is most welcome. Curiously, the article stresses another, weaker finding of differences between Labour and Conservative MPs. As shown by Searing's Table 5, party differences of 19% or more occur for 7 of the 26 questions (one difference of 17% is misprinted as 27%). A glance at the questionnaire and coding procedure (Appendix) suggests that the party differences are tied to invalid measurements.

For four of the seven questions with party differences, support for "rules of the game" is measured by support for *changes* in current practices.

> The Opposition should have better resources for factual briefing and information. (Support is "agree.")
>
> The concentration of power in the hands of the Prime Minister has gone too far. (Support is "agree.")
>
> There should be greater opportunities for Members to question Ministers publicly on the affairs of their Departments. (Support is "agree.")
>
> In a world as complicated as the modern one, it doesn't make sense to speak of increased control by ordinary citizens over Governmental affairs. (Support is "disagree.")

The measurements cannot be valid. Searing equates support for "rules of the game" with the perception that the Conservative Government was violating the Constitution. Most Conservative MPs supported change on two of the items (54%, 75%), yet continued to support the Government. Either they did not see the rules as a higher law above partisanship or they did not see the changes as involving the "rules of the game"—in either case, their apparent support is misleading. Similar problems occur in the interpretation of responses of other Members. Support for these changes cannot be equated to support for the "rules of the game."

Additionally, these questions are ambiguous. They do not state a clear rule, relying instead on direction and comparison: "better," "too far," "greater," and "increased." Support for a direction of change is not the same as support for a specific standard. Ambiguity plagues the other questions as well.

> Ministers should be prepared to reveal to Parliament virtually all information on their Department's affairs. (Support is "agree.")

The question text itself demonstrates the difficulty of posing the constitutional issue, as it relies on "virtually," "affairs," and "prepared" in lieu of a standard. Two MPs with the same attitude might provide different responses: "Agree (although of course not in situation X): and "Disagree (not in situation X)."

> The strength and efficiency of a Government are more important than its specific policies. (Support is "disagree.")

No action or arrangement is even mentioned in the question, so no rule can be inferred. Searing subsumes this question under norms for the "role of the electorate," which in turn is identified with "majority rule" and "responsiveness to public opinion" (p. 241). Perhaps the argument is that the British electorate is indifferent to weakness, ineptitude, and scandal in Government, hence these characteristics should not affect its power. Regardless of the argument, the question ambiguity precludes reliable inferences about support for "rules of the game."

When questions are ambiguous, respondents rely on attitudes to one of the objects mentioned in the question text to cue their responses. As Searing points out, party differences on these items correspond both to a difference in "partisan advantage" and to a difference in "political values." Labour members benefit from "greater" opportunities to question Conservative Ministers and might also support corresponding opportunities for Conservatives, if Labour were in power. Hence we would expect party differences on these questions if no "rules of the game" were involved. When ideals and self-interest converge, attitudes correspond. As the questions are ambiguous on the rules and as the assumption that support for the rules is directly tapped is logically or empirically insupportable, the party differences appear to reflect the perceived *absence* of a constitutional issue.

The final difference is the largest one, between Searing, on the one hand, and considerable majorities of both Labour and Conservative MPs, on the other hand. The Members agreed that "it is not dangerous, but essential that the Government reach its policy decisions in private." However, to Searing, support for the British Constitution requires a "disagree." If it were a constitutional rule, the standard of public decision making would have to cover all cases, rendering superfluous some of the other rules, briefings, and questions, for examples. Even a "disagree" response does not indicate support for such a rule, indicating merely that there may be situations in which privacy is not "essential." All we know is that Labour members were somewhat more likely than Conservative members to have such situations in mind in answering the question; we do not know how many of these would support a rule of publicized decision making (although I think that zero would be a fairly accurate prediction). Note that what Searing had in mind—which may be eminently defensible—is irrelevant: interpretation depends on what the members were thinking.

None of the questions showing sizable party differences validly measures support for the "rules of the game." These questions, after a "final step" of adding "item values within sets in order to construct the new variables" (p. 242), provide the basis for the article's analysis of party differences. Correlations, principal components, value and subgroup analyses are merely ways of restating and rearranging the percentages, hence are no more valid than the underlying percentages. When the questions are clearly valid, as in the case of "supremacy of Parliament," party differences are negligible. The stress on the weaker findings based on the error-prone measures obscures the more significant and trustworthy confirmation of our prior expectations.

DOUGLAS D. ROSE

Tulane University

Forthcoming in June

Claudio Cioffi-Revilla, "The Political Reliability of Italian Governments: An Exponential Survival Model"

Harold D. Clarke, Allan Kornberg, and Marrianne C. Stewart, "Parliament and Political Support in Canada"

Michael D. Cohen, "Conflict and Complexity: Goal Diversity and Organizational Search Effectiveness"

Richard L. Cole and David A. Caputo, "The Public Hearing as an Effective Citizen Participation Mechanism: A Case Study of the General Revenue Sharing Program"

Gregory W. Fischer and Mark S. Kamlet, "Explaining Presidential Priorities: The Competing Aspiration Levels Model of Macrobudgetary Decision Making"

Lawrence LeDuc, Harold D. Clarke, Jane Jenson, and Jon H. Pammett, "Partisan Instability in Canada: Evidence from a New Panel Study"

Russell J. Leng, "Reagan and the Russians: Crisis Bargaining Beliefs and the Historical Record"

Michael MacKuen, "Exposure to Information, Belief Integration, and Individual Responsiveness to Agenda Change"

Kenneth J. Meier and Robert E. England, "Black Representation and Educational Policy: Are They Related?"

Clifford Orwin, "The Just and the Advantageous in Thucydides: The Case of the Mytilenaian Debate"

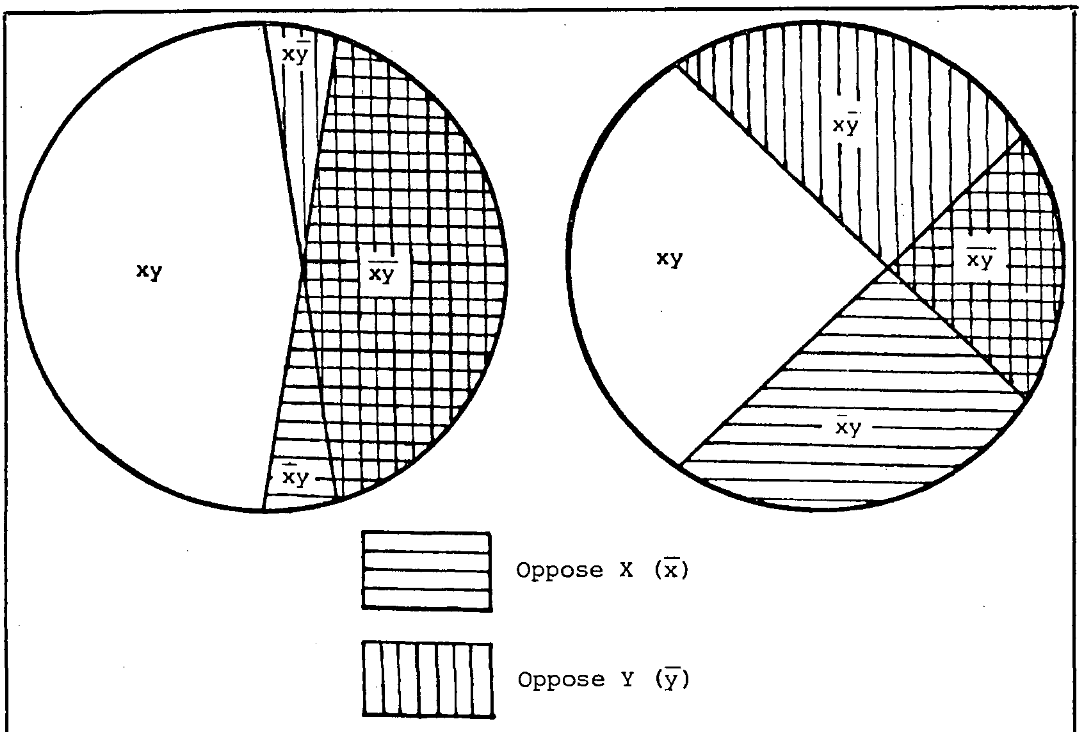
Kenneth A. Shepsle and Barry R. Weingast, "Political Solutions to Market Problems"

Michael Don Ward, "Differential Paths to Parity: A Study of the Contemporary Arms Race"

Erratum

In "Pluralism and Social Choice" by Nicholas R. Miller (Vol. 77, pp. 734-747), the correct labelling was inadvertently omitted from Figure 1. The correct figure appears below.

Figure 1.



I. Reinforcing Divisions

	55%	5%	5%	30%
first pref.	xy	$x\bar{y}$	$\bar{x}y$	$\overline{xy}$
last pref.	$\overline{xy}$	$\bar{x}y$	$x\bar{y}$	xy

II. Crosscutting Divisions

	35%	25%	25%	15%
first pref.	xy	$x\bar{y}$	$\bar{x}y$	$\overline{xy}$
last pref.	$\overline{xy}$	$\bar{x}y$	$x\bar{y}$	xy

BOOK REVIEWS

American Government and Politics

Political Attitudes in America: Formation and Change. By Paul R. Abramson. (San Francisco: W. H. Freeman, 1983. Pp. xxiv + 353. $14.95, paper.)

This book is an attempt to bridge the gap between a text and a research monograph reporting on personal research. Much of the material deals with somewhat arcane issues, such as whether life-cycle or generational changes best account for noted changes in attitudes and behavior and the Citrin-Miller debate, couched here as a debate over the Eastonian community, regime, and authorities distinctions. Students would be more attracted to the several chapters devoted to survey research and problems of question wording.

This is a current and thorough compilation of Abramson's work, with extensive citation from this perspective on what is happening to the American electorate in presidential elections since 1960. Three variables are treated exhaustively: participation in voting for president, identification with a polical party, and the trust/efficacy attitudinal complex. Three chapters are devoted to each concept: one on the development of the concept with extensive literature citation, one on its formation in youth, and one on changes among adults. Finally, there are chapters on tolerance, issue conceptualization, and the interrelationship between party identification and trust/efficacy changes and change in voting for president. Less ardent identification with political parties and the growing convictions that "officials don't care" and that people "don't have any say" are seen to largely account for the decline of voting for president. Although further implications of these views are discussed in the introduction and conclusion, no empirical evidence is offered.

Abramson limits his generalizations in the conclusion to the datedness of the findings in *The American Voter* (Wiley, 1960) and the importance of studying generational change. In the introduction, however, he quotes others, such as Curtis Gans, who in the *Washington Monthly* (October, 1978) wrote of Americans withdrawing consent from their leaders and the implications to democracy of such withdrawal. Certainly the case is not made in this book that the implications are particularly serious.

There are a number of problems even with the conclusion that less enthusiastic identification with political parties and increasing distrust efficacy account for participation decline. Many researchers have noted that the American electorate of the 1980s is much better educated than that in the 1960s, which given the strong tendency for the better educated to vote, should have greatly expanded participation. Introducing education change into consideration greatly complicates explanations. An additional complication comes from the fact that participation decline is neither much different between gubernatorial elections and national elections nor constant across states. Some states have shown very rapidly declining participation across elections and others, especially southern states, have shown increased participation. The orderliness of patterns shown by Abramson thus seemingly distort the actuality of our understanding of events.

NORMAN R. LUTTBEG

Texas A&M University

Television Coverage of the 1980 Presidential Campaign. Edited by William C. Adams. (Norwood, N.J.: Ablex Publishing, 1983. Pp. x + 197. $24.50.)

Like Adams's other volumes on media content research, this collection contains suggestive and thought-provoking observations about media coverage of political affairs. While focusing largely on familiar subjects, most of the 10 studies contribute new interpretations or new insight based on the campaign of 1980.

Anthony Broh's perceptive essay emphasizes the media's interpretive responsibilities in communicating results of opinion polls within a political system that highlights the role of public opinion in the nominating process. The media "commission and create, as well as report, poll findings" (p. 34). These polls reinforce candidate images as well as respond to them.

Michael Robinson and Margaret Shehan illustrate concretely with well-defined examples their contention that the wire service "reports," but television "mediates," analyzes, and evaluates.

Robert Sahr terms energy a "non-issue" in 1980 because it was used mainly as a campaign vehicle or a measure of candidate consistency over time. Joe Foote and Tony Rimmer observe that interviews and network anchor reports comprise one-half the national convention coverage.

Thomas Marshall establishes that three television networks and two national newspapers gave similar media attention to a key group of primaries, the results being a "homogeneous" picture of the nominating process. He develops a quantitative measure for "the verdict" of the media on each candidate. Diagrams show the verdict's relation to public opinion during the primary season.

Adams also expresses media effect in quantitative terms. He expands our understanding of elections since 1960 by discussing seven elements of media content; these help determine differences in media messages from television or newspaper. Then, using individual-level data from the Survey Research Center (SRC) in conjunction with his content analyses, Adams attempts to establish a comparative measure of media effect. He cross-tabulates voting behavior with answers to the SRC question, "What source do you rely on most for information about the campaign?"

Curiously, Adams's method violates his own warning that "confusing unique power with shared power can distort the entire subject of media impacts" (p. 163). He assumes that the respondents' answers to the media-reliance question can be used to measure the distinctive effect of television or print. The SRC surveys contain a second question, "Does the respondent watch news about the campaign on television or read about it in the newspaper?" In 1980, 90% of the voting respondents (85% of the total) reported watching campaign news on television; 79% reported reading campaign news in the newspaper. Adams must recognize that a large proportion of voting respondents are both television watchers and newspaper readers, despite their choices in the media-reliance question. His effort to separate the impact of one medium is less satisfying than other parts of the volume.

Robert Meadow's incisive analysis of the Carter-Reagan debate centers on its unspontaneous, rehearsed nature. He accuses the media of being "co-conspirators in the debate ritual" (p. 93) which is no more than an opportunity for candidates to offer "prepared remarks of the whistle-stop variety under the guise of a debate" (p. 91).

In the following essay Thomas Benson analyzes references to the candidates' hidden motivations in Bruce Morton's story of the debates. He thus explains television's implicit communication theory which "may or may not be a source of wisdom" (p. 113). Meadow, from another perspective, cites this televised story as the only report to mention the repetition of themes in the debate which Meadow located by word processor.

Describing solutions proposed in the early election-night projection controversy, Paul Wilson illustrates its almost unsolvable nature. Changing forms of election machinery have made the networks dependent on exit polls, a super-fast method for projection of winners.

The bibliographical information at the end of each chapter will be useful to scholars in this interdisciplinary field. The book is a distinct contribution to the growing body of literature on the media and American elections.

MARGARET K. LATIMER

Auburn University

The Individual vs. The Public Interest: Political Ideology and National Forest Policy. By Richard M. Alston. (Boulder, Colo.: Westview Press, 1983. Pp. xiii + 250. $20.00.)

Alston asserts that the goal of his book is "to stimulate readers to challenge existing policy prescriptions, and to engage in productive debate on the role of public and private forestry in the nation's forest management policy" (p. xii). I believe the author will stimulate those who read this book; however, the real challenge embodied in this book is directed at everyone who analyzes public policies and public policymaking.

The design of Alston's book implicitly challenges all public policy analysts to go beyond abstract normative theories and narrow technical approaches to policy studies. The book avoids these two extremes by combining an interpretation of the history of national forest policy, a discussion of recent legislation in this policy area, and a survey of social, political and economic philosophies that relate to forest policy. As the title of the book foreshadows, almost two-thirds of the book is devoted to the philosophic context of forest policy. Alston ranges over Plato, the Stoics, Martin Luther, Adam Smith, John Stuart Mill, Hobbes, Hegel, Pareto, and many other philosophers in the Western tradition to delineate the beginnings of the ideological cleavage that he believes exists among those who debate, formulate, and implement forest policy. He sees a fundamental clash between those who emphasize individualism and those who emphasize the community (the public interest) in developing forest policy. The general theme of his philosophic overview is that we must realize that this dichotomy is traditional and inevitable, but we must avoid developing ideological ruts based on individualistic and communitarian impulses. Alston believes the richness of the philosophizing that has preceded our era provides us with a resource for extricating ourselves from the current ideological routines.

In the current forest policy debates, Alston sees a continuation of the clash between private regarding and public regarding approaches. He identifies the three main interest groups stemming from this fundamental clash as the economists, the foresters, and the preservationists. The economists stress individual freedom in a market economy, the foresters demand professional management of the forest as a natural resource, and preservationists assert that nature's interest is an integral part of the public interest of human society.

Alston sees these three conflicting viewpoints converging on the question of the role of the state (the federal government) in forest policy. Although there is no method available to reconcile the philosophical differences between these groups, Alston believes the forest planning process contained in the Resources Planning Act of 1976 and the National Forest Management Act of 1976 provides a policymaking structure that will integrate the biological, social, economic, political, and cultural dimensions of forest policies and allow policymakers to transcend ideological postures while at the same time providing a forum for the ideological positions staked out by the three groups mentioned above. He believes the new integrated multiple use resource planning process will satisfy neither the individualists nor the communitarians but rather will recognize and accommodate the large bureaucratic organizations, public and private, that are needed to manage the forest policy process in the late twentieth century. Such large-scale organizations usher in a policymaking era in which the old shibboleths of individualism and communitarianism are more and more obsolete or irrelevant.

Alston is certainly not the first analyst to plot this line of analysis. However, I believe his book serves as a refreshing antidote for all of the atheoretical and ahistorical policy analyses that have been written. The primary weaknesses of the book are insufficient integration of the overview of philosophic traditions with contemporary forest policy dilemmas and the timidity of Alston's prescriptions for forest policy in the United States. The latter criticism may stem from my own desire for the comfort of the clashing of competing ideological ruts.

CRAIG RAMSAY

Ohio Wesleyan University

The Logic and Limits of Trust. By Bernard Barber. (New Brunswick, N.J.: Rutgers University Press, 1983. Pp. 190. $27.50, cloth; $9.95, paper.)

In examining the concept of trust as a social phenomenon Bernard Barber has focused on a relationship of fundamental theoretical and practical importance to democracy. Barber, a sociologist by training, argues that trust results from individual expectations in a wide variety of social contexts and "has the general function of social ordering, of providing cognitive and moral expectational maps for actors and systems as they continuously interact" (p. 19). Barber is interested primarily in two specific forms of trust. These are the expectations, where the social system anticipates such behavior, that another will provide technically competent performance and that the other with which one interacts will perform the duties of fiduciary responsibility, i.e., place broader social interests above his or her narrow interests. Barber uses these definitions of trust to explain the expectations that appear to be developing regarding marriage, philanthropic foundations, public office, business, and the professions of medicine, law, accounting, science, and helping services.

With his discussion of political life, rational, or realistic, distrust becomes an important variable in Barber's approach. He argues that the portion of the public often seen as apathetic and alienated is instead expressing rational distrust of both the technical competence and fiduciary performance of their public leaders. Unfortunately Barber never convincingly presents a case for the existence of rational distrust among the politically inactive public. His major effort in this respect occurs in the chapter on politics where, without a single specific reference to any statement of theirs, Robert Dahl, David Easton, and Gabriel Almond are labelled "democratic elitists" who see the nonpolitical masses as "ignorant, alienated . . . or anomic" (p. 72, also pp. 88, 92, and 169). By so characterizing these political scientists without supporting evidence of any kind, Barber gives the appearance that he is erecting a straw man as a means of giving more credence to his realistic, or rational, distrust view of the masses, an impression that is reinforced by his failure to show that the segment of the public that appears disinterested in politics is anything more than that. His data, as presented here, are attenuated at best. One of the two studies that he utilizes heavily draws extensively on a comparison of populism in Kansas and reform in Birmingham, England, during the 1890s (p. 72). The other is an interpretation of survey research data that seems so general as to be of little value to Barber, as when he quotes the "conclusion" that "the level of political alienation in the United States probably falls somewhere between 25% and 75%" (pp. 78-79). At another point he draws on an interpretation of an earlier interpretation of field research on teenagers (pp. 82, 84). It may be possible to show a widespread sense of realistic distrust of institutions among the masses, but Barber does not do so. And, given his treatment of political scientists and the importance of distrust to his perspective, it seems only fair to insist that he produce more convincing arguments and data on this point.

Despite these problems, Barber's book remains worth reading. He writes exceptionally well and makes consistent use of his definitions of trust to provide fresh perspectives on the social relationships that he examines. Political scientists interested in investigating such concepts as political obligation and legitimacy should find this book a useful beginning point for broadening their perspective and organizing their thoughts.

ROBERT A. HEINEMAN

Alfred University

Science Policy from Ford to Reagan: Change and Continuity. By Claude E. Barfield. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1982. Pp. xvi + 142. $13.95, cloth; $5.95, paper.)

Federal expenditures for scientific research and technological development have been on the increase since the end of the Nixon White House years, a trend which the Reagan administration has continued. However, federal science policy has changed much since 1974. Claude E. Barfield chronicles the shifting priorities and spending patterns for science and technology in the past three administrations. Although the book does little to critique the Ford, Carter, or Reagan approaches to science policy, it is valuable nevertheless as a coherent report of emerging federal policies in four areas: basic research, space, defense, and energy.

Barfield's organizational and evaluative thesis is that Reagan administration policies for science and technology originated with President Ford's *Issues '78,* a document which set down a "thorough rationale for federal R&D policy in his last month in office" (p. 1). This rationale for increased expenditure for scientific research and development was also embodied by the Carter administration, argues Barfield, thus providing the "continuity" in science policy referred to in the title.

But Reagan's campaign promises of decreased federal spending and regulation in all but defense matters have brought many changes in federal support for science. For instance, although the Reagan administration has increased expenditures for basic research, a greater proportion of that research is now done under the auspices of the Defense Department, rather than under the traditional aegis of the National Science Foundation or National Institute of Health. Barfield notes the obvious dangers of a defense-dominated basic research program, but concludes that there is "nothing sinister" about such a "strong role for the DOD in R&D" (p. 119). Many scientists and lay citizens would disagree strenuously with this assessment, particularly in light of the concomitant lessening of federal support for health-related research and science education programs.

What Barfield clearly admires in the Reagan science policy is its consistent adherence to the larger ideology of the administration concerning the role of government in American society and economics. The Reagan administration has replaced federal funding in many areas of research and development with incentives such as tax credits to industries that increase their role in research and development. This is in keeping with overall taxing and spending policies of the administration. But as Barfield points out, some administration actions, such as continued support for the Clinch River breeder reactor project, seem to contradict the fundamental administration bias against using federal money for large-scale technological demonstration and commercialization projects (pp. 114-115).

Continued support for the breeder is due largely to politics, as the project is sited in the home state of the Senate Majority Leader. Presidential politics being what they are, this is hardly surprising. What is surprising is how little Barfield discusses the political side of science policy decisions. This book treats such decision making as if the only relevant actors in science policymaking were the president and a few agency heads in the executive branch. One need only to pick up a newspaper and read about the latest controversy regarding nuclear power, the freeze, the disposal of hazardous waste, or any of a host of other issues, to realize that today most of politics is centered on science policy questions and vice versa. Ignoring the politics of science policy is not unique to Barfield's work of course; indeed, doing so has become something of an addiction for many who have made science policy their field of study. Unfortunately, Barfield's book does little to lessen the addictive power of the proposition that policy for scientific research and development can be made outside of the realm of politics.

RICHARD P. HISKES

University of Connecticut

Being Governor: The View from the Office. Edited by Thad L. Beyle and Lynn R. Muchmore. (Durham, N.C.: Duke University Press, 1983. Pp. 234. $29.75.)

The editors, who are also the principal contributors, rely heavily on questionnaire data, complemented primarily by interviews with 15 former governors, to characterize the several roles of the contemporary American governor. There are 18 chapters; many are less than 10 pages. The book is divided into assessments of the political, managerial, legislative, and structural roles of the governor.

Much of the narrative addresses the 39 tables that display questionnaire data. Thus, there is less texture to the writing style than in, for example, Alan Rosenthal's description of contemporary American state legislatures (*Legislative Life,* Harper & Row, 1981).

Being Governor should prove useful to teachers of state government. It should also be available in college libraries, for there are several chapters such as "A Day in the Life of a Governor," that would make good reserve desk assignments for state governments courses. In addition, candidates for governor and their prospective staffs would benefit from perusing the book, especially the several chapters that offer pithy lessons learned from former governors and their staff members. For example, staff members provide a list of "Thou shalt nots" for the appointment process; "Do not consult too widely; Do not delay, thereby letting pressures and expectations build; Do not let applicants or their supporters see the governor personally; . . ." (p. 117).

There are excellent chapters in this uneven book. Robert Dalton's chapter, "Governors and Ethics," is valuable. It helps us to appreciate the ethical ambiguities of gubernatorial and staff decisions in a setting where varied favor exchanges provide a useful way to facilitate action.

Perhaps the most illuminating chapter is by Muchmore on the topic of science advice to governors. In addressing the question of where a science advisor or science advice should be located in the executive branch, Muchmore makes broader observations. The governor's office should be viewed as "a separate organization that is distinct from the bulk of the executive branch" (p. 183). The heavy and conflicting demands placed on a governor cause most to become satisficers who will "expend time and effort to find an *acceptable* decision, but having done so will not commit additional resources to find an optimal decision" (p. 184).

The book displays the deficiencies associated with heavy reliance on questionnaire data. Did the governors respond for themselves, or did staff handle this assignment as well? And perceptions do not always represent reality. While two-thirds of the planning directors and budget officers indicated they would be involved in some way with decisions in reaction to a disaster, only 15% of governors' office staffs thought the planning and budgeting people would be involved (p. 178). This tells us much, but not who would be involved.

Nevertheless, *Being Governor* enhances our understanding of operations in the offices of American governors. The publisher is to be commended for yet another title in its policy studies series on state and local issues.

Beyle and Muchmore have a wealth of participant-observation experience with governors. One would hope they would now move from process to outcome questions critical to our beleagured governors and to the evolving federal system. Does executive reorganization really make a difference? Has the increase in executive branch capacity kept pace with increases in demands on governors? Can gubernatorial performance justify the increases in responsibilities imposed on the states in recent years?

JAMES D. NOWLAN

University of Illinois at Urbana-Champaign

Beyond the Wasteland: A Democratic Alternative to Economic Decline. By Samuel Bowles, David M. Gordon, and Thomas E. Weisskopf. (New York: Doubleday, 1983. Pp. 465. $17.95.)

This is an ambitious and formidable book. It attempts a comprehensive analysis of the current economic crisis in the U.S. which, the authors claim, can account for the productivity slowdown that has puzzled so many conventional economists. The authors go much further. They also develop, in considerable detail, a coherent set of proposals designed to show that "the trade-off between material security and a decent society is not a law of economics . . . [but] a veil of privilege" (p. 15). They contend that a democratic alternative is the "key to moving beyond the wasteland" (p. 15). In my view, the bold and ambitious project largely succeeds. Readers will be rewarded with a multitude of insights and new directions to explore.

One of the strongest aspects of the book is the authors' sustained development of a social model of the economy that is grounded in these simple but often forgotten (by conventional and Marxist economists) truths: the "economy is people [and] its basic relationships are social relationships" (p. 5). The rejection of a mechanistic view of the economy is accomplished without sacrificing intellectual rigor or technical sophistication. One should in particular commend the authors for constructing novel measures that reflect the social categories they employ in their analysis. And while one can quibble with the inclusion of one or another factor in the measures, one has to appreciate their determination to not allow conventional accounting procedures to either limit their analysis or condemn them to abstract theorizing.

Bowles, Gordon, and Weisskopf begin their analysis by arguing that U.S. economic growth from 1948 to 1966 was built on three institutionalized power relations between U.S. capital and (1) foreign competitors and suppliers; (2) a large segment of the labor force; and (3) citizens, as mediated by the state. These potentially conflict-ridden relationships permitted profit-led growth because capital was either strong enough to suppress challengers (for example, internationally) or because it bought off certain groups (for example, unionized labor). Beginning in the mid-1960s, the contradictions inherent within each of these three social relationships began to erupt into overt challenges to U.S. corporate dominance. For example, foreign competitors began to penetrate the U.S. market, which, when combined with other developments, began to undermine corporate profitability. Labor, emboldened by historically low levels of unemployment and the cushioning effects of social programs, pushed hard for increased wages, improved working conditions, and lowered intensities of work. Multiple coalitions of citizens achieved legislative victories that constrained the actions of corporations. According to the authors, these challenges to corporate dominance explain the decline in productivity and profitability in the mid and late sixties.

The next 15 or more years have witnessed a two-stage counterattack by capital. In the first stage (1973 to 1979) the primary objective was to weaken the power of labor. To that end management attempted to reassert control in the workplace by increasing supervision, speeding the work process, and by trying to frighten workers by actual and threatened capital mobility. At the societal level, the government engineered a deep recession to assist capital in their attack on labor.

However, while there were signs that labor was weakening, the forces of opposition had achieved such strength in the postwar years that only a further, intensified attack on worker and citizen power could reestablish the conditions for corporate dominance (hence, the Reagan program). But, the authors point out, these kinds of solutions impose immense social costs, built as they are on rules favorable to business and full of so many contradictions. For example, the deep recession of the mid-seventies worked at the same time to increase productivity by frightening workers into intensified work effort and to decrease productivity by reducing capital investment because of the unutilized capacity. According to their estimates, the U.S. economy wasted some $1.2 trillion in useful output in 1980.

Their solution builds on this simple premise: "it costs less to put people in charge than it does to keep them down" (p. 262). Hence, the authors propose a 24-point economic bill of rights and numerous policy proposals to empower citizens and eliminate demand- and supply-side waste. Although some will dismiss the proposals as utopian or too radical, they seem to me to be pragmatic, conceivably workable, and undogmatic. The authors detail the steps needed to get from here to there and are sensitive to the dangers of bureaucratic centralization. Indeed, the book is likely to be controversial not because it is too radical (although it is in the true sense of the word), but because among all the current blueprints for economic restructuring it is the only one unambiguously advocating more democracy as the essential ingredient of the solution.

LEON GRUNBERG

University of Puget Sound

The Limits and Possibilities of Congress. By Philip Brenner. (New York: St. Martin's Press, 1983. Pp. vii + 206. $15.95, cloth; $8.95, paper.)

The primary theme of Brenner's book is that Congress has fewer limits and greater possibilities than commonly assumed, although several other themes are also explored. Brenner takes a political economy approach to Congress and posits that environment (especially the ideology of the nation and its productive forces) is more important than member characteristics. Here he is on shakier ground; virtually no evidence is provided for such an assertion. There may be good reason why, as Brenner states, these "Marxist assumptions . . . do not enter traditional studies of Congress" (p. 17).

Brenner debunks constituency and other external and internal influences on legislative behavior and tries to discredit five assertions that he believes have dominated the literature: that congressmen are parochial, primarily seek reelection, focus on personal interests, are concerned with internal power, and in some cases have public policy concerns. As Brenner states, some of these alleged traits are paradoxical.

The arguments in this book are provocative, but the analysis is flawed. The study contains many statements left uncited and unconfirmed by references to nonscholarly interpretations. Brenner relies upon three case studies that he feels illustrate the dominance of the Marxist interpretation. Two cases deal with substantive policy issues (relations with Cuba and restructuring higher education), while the third concerns general congressional reform. Although the cases do show Congress playing a major role in both domestic and foreign policy, little other rationale exists for their selection. Neither the Cuba nor the higher education case produces clear evidence of the dominance of international or external contexts. In fact, the opposite arguments could be made, and Brenner himself states that "in the case of a non-crisis issue . . . legislators tend to act only to serve personal and institutional as well as national interests" (p. 88). The discussion on reforms is a good overview of the literature and the nature of internal changes, but Brenner admits that reforms have had both centralizing (presumably external) and decentralizing (internal) features. He concludes from this very mixed bag that "self-interest does not explain the pattern of reforms" (p. 166).

Most of the data that form the bases of the case studies come from interviews. However, the procedure is very loose; no mention is made of an interview schedule or sampling procedures. Regarding the latter, Brenner has confused response rate with sample size (p. 52). The sample of 61 House members on the higher education case seems more representative.

The presentation of data is also perplexing. Frequently it is unclear why particular variables are examined (for example, class and member ideology in Table 5-3). While the data do not warrant sophisticated empirical tests, no assessment is made of the relative impact of any of the variables. Aggregate data on the economy and the nation's beliefs are notably missing; after all, these external explanations are supposed to be superior to member characteristics such as ideology. For example, does the public share Brenner's assertion that Congress "often equates the common interest with the interests of those who own property [particularly] . . . corporations" (p. 33)? Periodically a poll is cited as evidence, but none are discussed over time, and some appear to contain flawed questions.

The Limits and Possibilities of Congress is a unique but limited book. With its policy, reformist, and philosophical treatment of Congress, it does make a contribution in emphasizing that Congress must be looked at in the context of its larger environment, but Brenner has not done justice to that environment. Further, the book's economic interpretation is of limited utility, with wavering arguments and logical gaps. In short, the book lacks credibility in viewing Congress either as an institution or as a participant in its larger milieu. It is reasonable to expect that important books, even though based on case studies, will at least build a framework for subsequent analysis; Brenner's book supplies no such structure.

STEVEN A. SHULL

University of New Orleans

Politics and Health Care Organization: HMOs as Federal Policy. By Lawrence D. Brown. (Washington, D.C.: Brookings Institution, 1983. Pp. xv + 540. $33.95, cloth; $16.95, paper.)

Politics and Health Care Organization is a well-written and detailed analysis of the interplay between politics and policy during the federal government's attempt to stimulate the development of health maintenance organizations (HMOs) during the 1970s. Brown establishes a very clear objective for his scholarly analysis by stating that the small federal HMO program was examined because it is "singularly revealing about the emergence of rationalizing politics in the 1970s" and the current quest of the federal government "for control over programs it had earlier and hopefully brought into being, a quest earnestly pursued today in health care and in other policy fields" (p. 16). The book is divided into three parts, each of which is thorough and well-documented enough to stand alone. Together the three sections are an excellent case study that clearly demonstrates the dynamics of the policy-making process and the difficulties of translating good policy analysis into effective public policy.

Part 1 examines HMOs as organizational entities. Brown analyzes the background of prepaid group practices or HMOs, their development in the U.S., their management problems, and the empirical evidence on their performance. He discovers that HMOs "run sharply counter to deep and fundamental trends in the organization and financing of care in this country" (p. 73). The discussion of the organizational and managerial problems of HMOs concludes that their management "is an inherently fragile enterprise" (p. 127). In chapter 4, Brown reviews the empirical studies assessing the performance of HMOs in balancing the three objectives of quality, access, and cost. He concludes that the decision to promote HMOs was based more on political factors than on the empirical evidence.

In Part 2, Brown provides a detailed account of the politics that produced the HMO development program in 1973 and subsequent amendments to the law in 1976 and 1978. The three chapters in this section are an excellent and interesting examination of the conflict, confusion, and compromise inherent in the public policy process. In this case, the interest group that allegedly was the beneficiary of the new program, the HMO lobby, considered the new law to be a "millstone around their collective necks," while President Nixon called it a "milestone" in the "administration's health strategy" (p. 267). Brown's review of the attempts of the Department of Health, Education, and Welfare to implement the HMO law demonstrates that "the administrative process was lengthy, cumbersome, and complex." Both the law and its implementation were plagued by the same problem: "no agreement existed among politicians and affected interests on what the federal HMO program was or should be, or indeed on what an HMO was or should be" (p. 344). Brown concludes this section by discussing the attempts to reform the HMO program and thereby enable it to expand into a viable alternative to the traditional system. His "reasonable inference" is that the HMO development program had "always been a highly implausible policy strategy" (p. 398).

The last section of the book is a well-reasoned explanation of why the program had very limited accomplishments. Chapter 8 solidly supports the hypothesis stated earlier: "the source of both the policy attractions and the political vicissitudes of the HMO program lies in its character as an organization-building strategy" (p. 26). Ironically our decentralized political system made the HMO strategy politically appealing, however it also made it unlikely that the strategy would be successful. Chapter 9 is an excellent examination of the major difficulty of policy analysis—translating a good idea into a good policy strategy. The concluding chapter looks toward the future of federal health policy by drawing upon the lessons of the HMO experience. Brown reviews the four general strategies used by the federal government to intervene in the health care system and then analyzes future policy options with an assessment of their likelihood of successfully containing health care costs. He concludes on a pessimistic, but possibly realistic, note: "No simple solution is in the cards; no tolerable strategy will end soaring medical costs" (p. 531).

Overall, Brown has produced an excellent, well-balanced analysis of the HMO development strategy. Furthermore, he uses a broad theoretical framework that provides many useful insights into the policymaking and administrative processes on the federal level in general, not just in health policy. There are some places where Brown has overstated his case, but they are not serious and add fuel to the debate over health care cost containment. In all fairness, his focus on the federal level repeatedly acknowledges the decentralization in the system, but his analysis does not adequately explore the option of state governments containing health care costs through the competition strategy. His pessimistic conclusion seems valid on the federal level, but perhaps it is time that we recognize that in some policy areas the state and local governments might be able to pursue reform and reorganization strategies successfully when the federal government can not.

RICHARD D. WILLIS

Ohio University, Athens

Shutdown in Youngstown: Public Policy for Mass Unemployment. By Terry F. Buss and F. Stevens Redburn. (Albany: State University of New York Press, 1983. Pp. xvi + 219. $39.50, cloth; $10.95, paper.)

This multidisciplinary study examines the 1977 steel shutdown in Youngstown, Ohio. The authors first consider background. In 1969, Youngstown Sheet and Tube Company was merged with Lykes Corporation, a conglomerate headquartered in New Orleans. Lacking experience in managing steel production, Lykes did little to prevent an accelerated decline of the Youngstown operation. It diverted resources away from steel production and, shortly after the merger, Youngstown Sheet and Tube began to accumulate huge losses. Eight years after acquisition, Lykes announced a permanent shutdown of a major steel facility and laid off 4,100 workers. When closure came, Lykes offered little help or even information. Except for meeting long-standing contractual obligations on such matters as early retirement, it simply abandoned its employees and the community. Unemployment compensation and related forms of government assistance cushioned what would otherwise have been a devastating financial blow for most families.

Because much of the book concerns mental health and human service issues, the researchers apparently expected to find that mass unemployment induced a high level of personal stress and social dislocation. Instead, they found only moderate stress, no signs of community upheaval, and little immediate impact on general social conditions. Unemployed steel workers behaved accordingly. Far from being radicalized, they displayed strong attachment to the conservative rhetoric of individualism and self-reliance. Beyond voting out of office their local union officials, they made few protests.

Context is important. The 1977 shutdown occurred in times of growth, both for the nation and the region. The local labor market proved to be "remarkably elastic or absorbent" (p. 94). Youngstown was a socially stable community, with extensive informal networks that the laid-off steel workers used to reenter the job market. An estimated 95% either got new jobs or took early retirement. The authors note that area employers appeared to "skim off" the more experienced and skilled workers, thus displacing unemployment onto others. From all indications, the inexperienced and unskilled, not former steelworkers, were the principal victims of a shrinking job market. As the full picture is painted, the absence of community upheaval becomes understandable. The impact of the steel shutdown was muffled by the general context of economic growth; its consequences were delayed and dispersed. Indeed, these circumstances leave the Youngstown case with limited comparability to current experiences of deep unemployment in the Midwest.

The book has the strengths and weaknesses of a multidisciplinary approach. It covers many facets of the situation but lacks sharp focus and theoretical depth. For that reason many of its policy recommendations are unconvincing. Indeed, the policy problem itself is never defined clearly. The authors describe the total ineffectiveness of social services, but fail to look for underlying causes of that ineffectiveness. For services that are irrelevant, the remedy must be stronger than a call for less fragmentation and better outreach. Put another way, the search for adequate counseling appears not to offer a solution to the problem of declining industrial base.

The main dog that doesn't bark in this story is collective political action. To be sure, there was an Ecumenical Coalition formed, and local governments heightened their organizational attention to economic development programs. But no one was able to mobilize mass support. The book, I regret to report, gives only an abbreviated account of these efforts. Buss and Redburn conclude with some tantalizing discussion of the connection between atrophying political life and absentee-owned corporations, but the politics of aging industrialism remains a story largely untold.

CLARENCE N. STONE

University of Maryland

Policymaking and Politics in the Federal District Courts. By Robert A. Carp and C. K. Rowland. (Knoxville: University of Tennessee Press, 1983. Pp. xiv + 203. $17.95.)

For several years Robert Carp and Claude Rowland have been two of the most astute students of judicial behavior on the federal district courts, and this book presents their most important and systematic findings to date. The time period considered is 1933 through 1977 for district court appointees from the Wilson through the Ford administrations. The data base is a staggering 27,772 sample decisions. These data are disaggregated into five categories: criminal justice, government and the economy, support for labor, class discrimination, and First Amendment rights, with 20 subcategories used for analysis at various points in the book. The research design is sound, the approach to data collection and classification is solid, relatively simple models are employed to generate meaningful hypotheses, and the statistics applied (ranging from simple percentages to regression analysis) are explained clearly.

The fundamental purpose of the book is to determine, even if somewhat tentatively at times, the relationship between liberal-conservative voting tendencies of federal district court judges and their party affiliations, the appointing presidents, geographical variances, and levels of urbanization. Individual chapters examine each of these independent variables after an overview of related literature. The basic findings may be summarized as being, for the most part, what one would expect.

Carp and Rowland find that Democrat judges on the district courts have been more liberal than their Republican counterparts in all but one of the 20 issue subcategories. Partisan voting differences varied over time but were the greatest between 1968 and 1977. Carp and Rowland thus conclude that party identification is now a stronger independent variable than has been suggested by prior studies of district court voting. The ideological impact of appointing presidents on district court decision making has also fluctuated longitudinally. Johnson appointees have been the most liberal since the Wilson administration based on the aggregate data, whereas the Nixon and Eisenhower appointees have voted most conservatively. The ideological impact of Truman and Kennedy on district court decision making has been relatively minor by contrast. Federal trial court judges in the North and South have also differed in their voting behavior. For example, since 1968 northern district court judges have been generally more liberal than their southern counterparts in cases involving criminal justice, civil rights, and civil liberties. As one would anticipate, there are exceptions to these trends, and variations exist within circuits and states. Regarding the urbanization variable, the greater the number of judges assigned to a district court, the greater the tendency toward liberal voting. However, broadly speaking, the relationship is stronger for urban judges in the South as opposed to those in the North.

Overall, the Carp and Rowland volume constitutes an excellent piece of behavioral research that also emphasizes politics on federal district courts and the judges' policymaking environments. Beyond voting patterns, the authors occasionally rely on quotations from judges interviewed, thereby adding color to the book and insights into judicial perceptions. The book will enlighten many who tend to believe that behavioral research is rather boring and many who think that the exploration of appellate court behavior is more interesting than that on trial courts. In taking the discipline beyond earlier studies and laying a foundation for future research, Carp and Rowland have carefully shaped a book that should be read by all those interested in American judicial behavior and process.

CHARLES M. LAMB

State University of New York, Buffalo

Interest Group Politics. Edited by Allan J. Cigler and Burdett A. Loomis. (Washington, D.C.: Congressional Quarterly Press, 1983. Pp. viii + 373. $10.25, paper.)

This is an era of vigorous activity on the part of interest groups in American politics. In the past two decades, we have witnessed a tremendous expansion in the volume of demands upon Washington made by pressure groups. While the media have made much of this development, political scientists in the past decade have paid it less heed, largely neglecting the subjects of lobbying and pressure group politics. To their credit, the editors of this volume have gathered together a selection of essays from the thin body of contemporary work on the subject.

The articles assembled here are varied and timely. Among the subjects these chapters cover are: the efforts of pro-life groups in the 1980 Senate campaigns; the political impact of the Christian Right; the evolution of the American Agriculture Movement from an ad hoc protest movement into a formal interest group; the transformation of the women's movement into a formidable lobbying force; the role of PACs in congressional campaigns; the nature of contemporary grass-roots lobbying campaigns; the litigation strategies of civil rights organizations; the importance of congressional caucuses as in-house lobbying forces; and the nature of interest group politicking in the executive branch. It is no particular slight to the other contributors' talents to say that the most interesting selection in the book is Elizabeth Drew's fascinating portrait of Washington super-lobbyist Charls E. Walker.

Unfortunately, when taken together, the essays are largely disappointing. There are two principal problems here. First, the volume is imbalanced in terms of the kinds of individual groups it examines. Its chapters focus largely on membership organizations of one sort or another, even though the world of Washington representation is increasingly dominated by corporate and institutional interests that face very different kinds of organizational maintenance problems and constitute a very different kind of lobbying force. Second, what at first appears to be a promising smorgasbord of selections turns out to contain little food for thought. The editors' own introductory chapter nicely sketches some of the major changes in interest group politics in recent years, but it does little to alert students to the intellectually interesting questions about the subject. And of the other three chapters meant to provide an overview of interest group trends and theory, only Robert Salisbury's speculative closing essay is especially thought-provoking. In fact, Salisbury is among those who insist that if we are to improve our understanding of organized interests in American politics, we must "systematically distinguish institution-based interest activity from that deriving support from individual citizens" in order to see "the former's greater staying power and long-term effectiveness in Washington" (p. 365).

The beauty of this book is that it exists at all. Any new literature on the role of organized interests in contemporary American politics is a welcome addition today. If the selections in this volume present a picture that is imbalanced and incomplete, it is because this is a field of inquiry that for so long has been neglected. It is a field ripe for new empirical and theoretical investigations.

JOHN T. TIERNEY

Boston College

Covering Campaigns: Journalism in Congressinal Elections. By Peter Clarke and Susan H. Evans. (Stanford, Calif.: Stanford University Press, 1983. Pp. xii + 151. $17.50.)

What does campaign reporting have to do with the rise of PACs, the decline of parties, the perpetuation of the incumbency advantage, and the growth in the number of turned-off voters? Clarke and Evans provide some answers in this compact volume and raise some important new questions in the process. The 1978 National Election Study included a collection of newspaper clippings about the House races in sample districts. This book is the most substantial presentation of findings to date about how American newspapers cover congressional campaigns, and with what consequences. Clarke and Evans report on the amount and nature of coverage upon voter perceptions of candidates. An important component of the research is a set of preelection interviews with 82 House campaign reporters.

Clarke and Evans find a pattern of coverage that serves to enhance the advantages of incumbents over challengers. For example, when reporters cover incumbents they tend to make more extensive use of the information they gather than when they cover challengers. They also exhibit a fascination with "political attributes" (experience, achievements) and an indifference toward issues. Challengers usually have little to offer in the way of experience, but a lot to say about issues. On the whole, campaign coverage of all types of House candidates is routine and unimaginative.

The extent of the incumbents' advantage in volume of coverage is impressive. One especially striking discovery is that the incumbent edge is largest in the most competitive races, precisely the settings where evenhanded press treatment might make a real difference in the outcomes. Open-seat races, even some lavishly funded ones, are given modest coverage. Incumbent walkovers are typically accorded next to no coverage. Although the data on amount of coverage is valuable, the authors might have strengthened the analysis with additional discussion of the nature of the coverage. How favorably or unfavorably are candidates portrayed?

Editorial endorsements are found to be typically thin and uninformative, and virtually all endorsements go to incumbents (even when editors and publishers have reservations). Coincidentally, endorsed incumbents tend to enjoy an enormous coverage advantage over their challengers. The endorsement process itself is depicted here as all too often an exercise in thoughtless ratification of the status quo.

Clarke and Evans devote an ambitious chapter to analyzing the 1978 survey data in order to ascertain the effects of press coverage on voter understanding. They tell us that the amount of coverage is positively associated with voter awareness, and that more favorable perceptions stem from frequent exposure to mentions of the incumbent. One reason that this chapter is a bit disappointing is that Clarke and Evans are unable to shed much light on the nature of the candidates' campaign efforts, other than as the papers report them.

The final chapter synthesizes the responses to the empirical findings of a group of inadequately identified media and political professionals. The result is a series of recommendations about how to improve House campaign coverage. One especially important suggestion is that issues need to be covered more extensively and more attractively if voter interest is to be revived and if the excessive incumbency advantage is to be checked.

This is a valuable book that should attract the serious attention of, among others, congressional elections scholars, who sometimes underestimate the role of the press in the electoral process. Certainly Clarke and Evans could have given us a richer brew of detail, example, and anecdote when describing the content of coverage, but the book is eminently readable and even entertaining. Perhaps, as Clarke and evans acknowledge, the 1978 House elections are an insufficient base for appraising subpresidential campaign journalism. Future work should certainly include analysis of Senate and gubernatorial races. We now have a benchmark from which to proceed.

CHARLES M. TIDMARCH

Union College

Feminism and the New Right: Conflict over the American Family. By Pamela Johnston Conover and Virginia Gray. (New York: Praeger Publishers, 1983. Pp. xv + 253. $29.95.)

It has become an adage of political science that "organization begets counter-organization." Nowhere is this principle better illustrated than in the emergence of the New Right to challenge the feminist movement on the Equal Rights Amendment and legalized abortion. Not for the first time, a split has developed between scholars and practitioners concerning the strengths of this new political force in American politics. Elected officials and activists, as well as the mass media, have viewed the New Right as a movement with a large following and significant power in the electoral and policy arenas. In contrast, political science studies have concluded that the impact of the New Right in elections and in general is highly overrated. Conover and Gray attempt to determine which perspective is the more accurate and, in doing so, provide a number of insights into the origins, mobilization, organization, and impact of these two movements, as well as non-class-based political movements generally.

The authors creatively use an eclectic data base —national and state opinion polls, aggregate state data, and interviews conducted with delegates to the White House Conference on Families (WHCF)—to look at the relationships between the key issues of abortion and the ERA and the opposing movements that have emerged to contest them. In general, their findings concerning the balance of political power between the two movements support those of earlier, more limited, scholarly studies. They find that the New Right is not as mobilized as feminists on the state level. Further, this new conservative movement has thus far had only a modest impact on state policy decisions. And, contrary to popular perceptions, single-issue voting is shown to be far more difficult, rarer, and a potentially stronger weapon in the hands of feminists, given the present distribution of opinion.

A second focus of the study is on the role issues and their attached symbols have played in the feminist and New Right movements. Conover and Gray extend the concept of "symbolic politics" by incorporating public opinion and interview data to support the thesis that feminists, by using the condensational symbols of equality and personal autonomy on behalf of the ERA and legalized abortion, not only successfully built their own movement, but also sparked the emergence and defensive mobilization of the pro-family sector of the New Right.

Conover and Gray also provide succinct analyses of both movements from the resource mobilization perspective of social movement theory. In addition to the ideological and organizational differences delineating an offensive (feminist) movement from a defensive (conservative) one, theoretically important commonalities in political tactics and basis of mobilization also exist.

Despite its title and broad scope, this will not be the definitive comparative study of feminism and the New Right. It is a well-executed and imaginative piece of research using existing or readily accessible data. And as the first book-length treatment of the New Right using survey and other statistical techniques, it should, and undoubtedly will, be widely read. Specialists may quibble with some aspects of the study. Historians would argue that, contrary to the assertions of the authors, the debate over woman suffrage was fraught with much religious and moral symbolism. Feminist theorists may feel that Conover and Gray overstate the tensions between the individual and the family in feminist thought. While acknowledging the constraints surrounding data collection, there are still some puzzling or weak choices of indicators (for example, the use of two very different groups of feminist organizations—those registered in 1980 as lobbyists or organized into PACs and those, including apolitical local feminist groups, listed in an almanac in 1974—as base points for measuring changes in feminist mobilization). Conover and Gray may also place too much credence in their correlates of pro-family mobilization in the states, given the low variance and small number of such groups (on average, less than five per state). More seriously, those WHCF delegates who supported both the ERA and abortion were drawn primarily from the ranks of the "helping professions," and only infrequently were members of liberal, much less feminist, groups. This greatly reduces the usefulness of the interview data, upon which a significant portion of the study relies, for analyzing activists within the feminist movement, a weakness of which the authors are fully aware. But aside from these few problem areas, Conover and Gray have produced a fascinating study that provides some much-needed, solidly based information on the New Right, its impact, and its opponents.

JANET K. BOLES

Marquette University

National Conventions in an Age of Party Reform. By James W. Davis. (Westport, Conn.: Greenwood Press, 1983. Pp. xxi + 304. $35.00.)

James Davis follows a long line of supporters of the prereform political parties and national conventions in attempting to argue the merits of the old presidential nominating process and disclose the flaws of two waves of party reform: progressivism and the efforts of "participatory Democrats and political amateurs." To accomplish this task, Davis provides a listing of the functions performed by the national convention (chap. 1), a history of the national convention (chap. 2), a description of the structure and process of the convention (chaps. 3-6), an analysis of the effects of television on the nominating process (chap. 7), and a discussion of national convention reforms (chaps. 8-9). There is little that is new in this effort, and the book is almost wholly descriptive rather than explanatory. It thus reflects the state of research on nominating conventions and stands primarily as an update of other encyclopedic treatments such as *The Politics of National Party Conventions* by Paul David et al., (Brookings Institution, 1960).

Unfortunately, much of the research on national conventions is filled with anecdotes about past conventions and campaigns, with minor attempts at theory-building. Davis continues in this vein with interesting anecdotes, the significance of which is often unstated. For example, the Ripon Society's challenge to the Republicans' victory bonus is chronicled through a series of court decisions, but the importance of the final decision of the full appeals court ("that courts should be 'slow to interfere with the internal processes of political parties. . .' " (pp. 55-56)) is neglected.

Davis provides a list of manifest and latent functions performed by the national conventions that could have helped him move beyond mere description and detail. With respect to one of the major concerns of the book, he could have shown how changes in the nominating convention produced changes in the performance of these functions. Also, the succeeding chapters on structure and process and the media could have been used to demonstrate how these factors facilitate or hinder the performance of the nominating convention's functions. Instead, the functions are utilized primarily to distinguish between the pre- and postreform periods. Thus, Davis neglects a major structuring (and theory-building) device.

Furthermore, given the state of scholarship in the area of nominating conventions, it would have been interesting if Davis could have integrated recent related research into the study. Since the decline of the traditional party is one of Davis's primary concerns, and one of the major concomitants of that decline is the proliferation of single-interest pressure groups, he might have utilized some of the recent research on interest groups.

The chapter "The National Convention in the Age of Television" indicates a glaring gap in political science research on the role of the mass media. There is very little direct research on the impact of television on opinion formation toward the candidates during the nominating process or television's impact on citizens' orientations toward the national convention. Davis uses the available research fairly well, but he could again cite and utilize a growing body of related research on the topic of the mass media and public opinion.

The final object of the book is to defend national conventions in the face of proposed reforms like national primaries, regional primaries, and other various delegate selection plans. The treatment of this topic is better than the previous chapters on the structure and process of the convention. Davis considers most of the proposed reforms and provides some insightful analysis of the reforms and their possible consequences. He is most perceptive when noting some of the conflicts between elected officials and party organization members that could be produced by some of these reforms attempting to institute party responsibility. Unfortunately, he fails to note that a similar division would occur between committed and uncommitted delegates as a result of his suggested reforms. Davis's discussion of the reforms omits recognition of the past abuses of the more "closed" system of delegate selection. Representativeness is used in extremely narrow terms, and the earlier virtual exclusion of blacks, women, and other groups receives short shrift.

In sum, *National Conventions in an Age of Reform* combines the best and worst attributes of the literature on nominating conventions. There are lucid descriptions of the nominating apparatus and procedures, but little explanation or development of hypothetical propositions. Without research that develops and tests such hypotheses, it will be difficult to evaluate intelligently the effects of the reforms discussed by Davis.

PATRICK A. PIERCE

Saint Mary's College

Sexual Politics, Sexual Communities: The Making of a Homosexual Minority in the United States, 1940-1970. By John D'Emilio. (Chicago: University of Chicago Press, 1983. Pp. xiii + 257. $20.00.)

In order to understand the gay movement—and indeed any political movement—one must examine the larger social, cultural, religious, economic, and demographic context. The context socially constructs the reality of homosexuality and the gay movement in America. There were no homosexuals but only homoerotic acts prior to the late nineteenth century. Industrialization and urbanization weakened the social system, providing the space for homosexuality to develop.

World War II furnished a crucial catalyst in that development. The massive mobilization, the disruption of the traditional social patterns, the predominately single-sex emotional attachments formed by the war; and, especially for women, the strict sexual segregation and the popular stereotype of WACS provided what D'Emilio terms as "something of a nation-wide coming out experience" (p. 24).

Although the changes were liberating, they also made gays more vulnerable to attacks. The cold warriors linked homosexuality with the communist threat. Although these attacks fostered a negative self-image for gays, the publicity and the labeling of homosexuality conferred meaning to unrecognized feelings.

In those repressive times, the Mattachine Society and the Daughters of Bilitis were founded and grew ever so slightly. The Communist party experience of the Mattachine founders influenced both the ideology and organization of the Society. Homosexuals were a minority with a false consciousness. Therefore, the first steps toward liberation were educational, fostering a positive self-awareness and a group consciousness. The Mattachine Society reflected both its roots and a reaction to the repression with a centralized and hierarchical structure and a secret leadership body. Relatively early, the radical founders were overthrown. The new leaders created a democratic structure; supported professionals in their study of homosexuality; pushed accommodation; encouraged good citizenship as defined by heterosexuals; and established discussion groups, not for consciousness raising, but for therapy. The Daughers of Bilitis developed a self-help orientation with an emphasis on respectability and a hostility towards butch lesbians and bars. This emphasis on respectability, however, accomplished little attitudinal change. It was a futile attempt to make that which society "defined as beyond respectability" (p. 125) respectable.

In the 1960s the black civil rights movement stimulated a new gay militancy on the East Coast. Meanwhile in San Francisco, the worlds of the gay bar and gay political movement began to coincide. The harassment of gays, a drag entertainer's candidacy for city supervisor, and the Tavern Guild —a political organization of gay bar owners and employees—broke the barrier that existed between the bars and the political organizations. Sexual community and sexual politics supported each other.

By the end of the 1960s, gay groups were successful in repealing antisodomy laws in some states, limiting police harassment in some cities, creating positive publicity, and gaining new allies. But they could not mobilize a large proportion of the gays.

D'Emilio has written *the* history of the American gay movement. He has collected and presented significant information on that movement, and he has neatly utilized the concept of a socially constructed reality. It is unfortunate that he ignores the scholarly literature on social movements, because his factual evidence conveys a serious but unstated challenge to the standard view of political movements. That view states that when strains occur in the normal functioning of society, people become discontent. If discontent breaks a threshold, then a political movement is born. The alternative view, resource or mobilization theory, is strongly supported by D'Emilio's study. A political movement begins when there is a sufficient accumulation of resources. Gay participation in other movements furnished political experience and leadership training. The civil rights and new left movements provided an ideology and a model of tactics. The gay bar fashioned the solidarity and the community in which the mobilization occurred. The gay movement developed when the resource level broke a critical threshold. Thus, it is important for political scientists to read D'Emilio's book both for its insights about the gay movement and also for its insights on political movements in general.

David C. Colby

Williams College

Technologies of Freedom. By Ithiel de Sola Pool. (Cambridge, Mass.: Harvard University Press, 1983. Pp. 299. $20.00.)

Ithiel de Sola Pool, Ruth and Arthur Sloan Professor of Political Science at the Massachusetts Institute of Technology, views the electronic communications revolution as one of potential promise for freedom of speech but offers both historical and current caveats to that possibility. Perhaps the book's genesis—it consists of work originally prepared and presented as Phi Beta Kappa lectures, as an article in *Daedalus,* and as coauthored Project '87 research as well as some material first presented here—can account for its uneven presentation of the historical development of freedom of speech.

The early chapters vary in quality, especially those which deal with the history of print media and the various collisions between print and the First Amendment. The specialist may find unnecessary detail; the generalist not enough. The discussions of electronic media and the First Amendment also move in and out of historical focus, sometimes so often that de Sola Pool's point is missed entirely.

Perhaps the major difficulty with the work is that de Sola Pool sets out to prove what might be unprovable. He states that "civil liberty functions today in a changing technological context. For five hundred years a struggle was fought, and in a few countries won, for the right of people to speak and print freely, unlicensed, uncensored, and uncontrolled" (p. 1). New technologies, he argues, might remove or otherwise endanger those freedoms, because what may masquerade as regulatory traffic control may, in fact, be a form of censorship. Without doubt, such charges can and will be cogently made regarding various electronic media including radio, television, and the new cable systems. Yet if regulatory agencies are to act in the public interest, there must be some kind of qualitative decision about what best serves the public interest. Too, it is the public's right not to receive unsolicited material. De Sola Pool does not seem to agree with the postal laws that forbid the delivery of unsolicited obscene material, calling them instead a form of censorship. By analogy, then, he sees regulatory efforts to remove or restrict that which does not approach community standards as a restriction of free speech.

Yet if the individual's rights not to receive obscene materials are absolute, then the regulatory agencies must protect those rights without restricting the ability of purveyors of pornography to sell their wares to those who find them of interest. In many respects, radio and video here become more like billboards and less like the pamphlets and papers that de Sola Pool cites for his argument. Therefore, what he calls "the preferred print model" (p. 245) does not apply.

One might quarrel with de Sola Pool's individual "Guidelines for Freedom" (pp. 245 and following): First, the First Amendment applies equally to all media; second, anyone may publish as he wishes; third, law enforcement cannot be brought about by prior restraint; fourth, regulation must be the last recourse; fifth, eventually there may need to be interconnection among or between common carriers; sixth, "recipients of privilege may be subject to disclosure" (p. 248), that is, private corporations may be required to give up information presently withheld in return for licensing privilege; seventh, such privileges should be of limited duration; eighth, regulatory agencies should not get involved with what a circuit is used for, merely with the fact that it is used; ninth, traffic controlling regulations may not be used where unnecessary, as with cable systems; and, tenth, copyright enforcement and understanding must be reworked to allow for the potentials of electronic publishing.

PHYLLIS ZAGANO

Fordham University

Restructuring the City: The Political Economy of Urban Redevelopment. Edited by Susan S. Fainstein et al. (New York: Longman, 1983. Pp. viii + 296. $27.50, cloth; $14.95, paper.)

This comparative analysis of the political economy of urban redevelopment builds on case studies of five U.S. cities: New Haven, Detroit, New Orleans, Denver, and San Francisco. More than one-half of the book is written by Susan and Norman Fainstein, who have included chapters by Richard Child Hill, Michael P. Smith, and Dennis Judd. Its focus is the transformation in the politics of land use, economic growth, and housing accompanying the social and economic restructuring of U.S. cities and the extent to which successive urban regimes and their policies benefit or disadvantage lower-income residents.

The case studies treat a variety of urban redevelopment issues: the politics of urban renewal and the Community Development Block Grant program in New Haven, the impact of the automobile industry on the political and spatial economy of Detroit, uneven development and the political struggles between pro-growth and preservationist forces in New Orleans, central business district redevelopment and urban sprawl in Denver, and the changing role of the local state in the politics of urban redevelopment in response to community mobilization in San Francisco.

The volume is not explicitly directed toward urban policymakers, but it has direct policy relevance. The Fainsteins claim that the local state does not necessarily play an active role in urban redevelopment efforts. The requirements for such action include national programs, administrative structures, and funding for redevelopment; substantial economic and racial change in the urban population; and a pro-growth political force (organized by the mayor, business community, or both) which has considerable popular support. It seems, then, that urban redevelopment policy is not independent, but rather is significantly shaped by national political alignments, investment patterns, and economic and urban policies, as well as by international political-economic forces. Further, the broad similarity in urban redevelopment strategies in U.S. cities suggests either that (1) structural limits severely constrain urban redevelopment policies, (2) innovative redevelopment programs face legal and institutional (as well as obvious political) obstacles, and/or (3) urban policymakers tend to lack sufficient imagination.

It also appears that urban redevelopment policies have been a qualified failure. Generally, the contributors hold that these policies have (1) not reversed dominant trends in the national urban system nor exempted any city from those trends; (2) consistently generated substantial political opposition and conflict; (3) heightened and displaced class and racial inequalities; and (4) built upon joint public-private coalitions which have served the accumulation needs of capital, socialized the costs of urban redevelopment, and reduced the democratic accountability of city governments and redevelopment policymakers. Indeed, market forces have redeveloped some city downtown and neighborhood areas in the absence of state initiatives. A case could be made that urban redevelopment policies tend, especially in the absence of working-class influence, to support probable marketplace "winners" and, thus, accentuate uneven development.

This book is unusual in that it remains uniformly good throughout, in spite of the large number of contributors. Nonetheless, the most significant contribution to the literature in urban political economy is clearly the Fainsteins' concluding essay. They characterize urban regimes as directive, concessionary, and conserving. This corresponds roughly to (1) state-planned urban renewal with little opposition, (2) renewal with community mobilization winning greater responsiveness to lower-class interests, and (3) private redevelopment facilitated and subsidized by a local state which reduces but does not eliminate lower-class representation. They also analyze the terms of conflict which distinguish redevelopment policy: (1) divisions within the middle and business classes (such as uncontrolled growth versus historic preservation), (2) the allocation of public investment between the downtown and residential neighborhoods, and (3) the allocation of state expenditures for capital accumulation or social consumption. They conclude that lower- and working-class interests, when mobilized, can achieve better representation in, and more benefits from, urban redevelopment policies. However, the structural bias of the local state is toward programs that enhance capital accumulation, reestablish middle-class and business control over strategic and desirable urban territory, and insulate city politics and urban development policy from popular influence. This, finally, suggests key principles in a socialist program for urban America: redistribution, social integration, and democratic participation.

CHARLES E. ELLISON

University of Cincinnati

Ideology and Abortion Policy Politics. By Marilyn Falik. (New York: Praeger Publishers, 1983. Pp. viii + 228. $27.95.)

The Abortion Dispute and the American System. Edited by Gilbert Y. Steiner. (Washington, D.C.: Brookings Institution, 1983. Pp. x + 103. $6.95, paper.)

The abortion question continues to attract considerable scholarly attention because it does not fit neatly into conventional theories of politics. The unequal distribution of economic goods and the resulting have/have-not cleavages are assumed to be the basis of political conflict in the standard treatises on politics, an assumption which contemporary national politics have, for the most part, supported. Occasionally, though, national issues emerge that seem to reflect more competing concepts of morality than competing property interests. The emergence of issues such as abortion raises questions about what forces bring these noneconomic conflicts to the forefront and the effect such conflicts have on the political process.

Ideology and Abortion Policy Politics represents an effort to identify those forces that have made abortion a national political issue. Falik's introductory chapter criticizes standard treatments of American politics for denigrating the importance of ideology as a motivating force. Her central thesis is that activists on both sides of the abortion question are ideologically motivated. "Pro-life" activists derive their positions from a "conservative-traditional-sacred" ideology, and "pro-choice" activists from a "liberal-modern-secular" ideology. These two ideologies, according to Falik, provide "hypothetical constructs that provide a means for organizing the several facets of the abortion controversy with a view to the underlying normative motivations for collective social and political action" (p. 39). Using data collected from interviews with 50 nonrandomly selected pro- and antiabortion activists, Falik shows the relationship between views on abortion and other policy positions and discusses the underlying motivational forces that cause the activists to take the positions they do. That abortion activists are ideologically motivated is undoubtedly true, but the evidence presented by Falik hardly constitutes the devastating indictment of the "end of ideology" thesis that she suggests. Curiously, Falik calls into question the integrity of the ideological belief system of the pro-life activists by characterizing them as exhibiting the "paranoid style in American politics" (p. 51), a style based on exaggerated fears of conspiracy and reflecting personal status uncertainty more than an objective assessment of real problems.

This study, which is being published in Praeger's Landmark Dissertations in Women's Studies series, was completed in 1975, so there is nothing in its analysis that examines recent survey and voting behavior studies, studies which have shed considerable light on the ideological dimensions of the abortion controversy. Falik's summaries of her interviews with abortion leaders provide some interesting insights into their thinking, but the data base she uses is too limited and too dated for this study to constitute a major contribution to the literature on abortion politics.

A more valuable volume is the collection of essays in *The Abortion Dispute and the American System.* The essays, originally presented in November, 1982, at a Brookings Institution symposium, address the question of what effect the abortion controversy is having on the political process. The general conclusion of the symposium's participants is reassuring: There is little in the abortion controversy that is without precedent or that is particularly threatening to the political process.

Lawrence M. Friedman, in his chapter on the Supreme Court's handling of the abortion question, argues that the *Roe* decision was legitimate in the sense that it built upon a preexisting legal foundation, but that efforts to overturn it by law or constitutional amendment are also legitimate. Since it was the Court that put the question into the Constitution (in an unsuccessful effort to get it out of the political arena), "opponents have no choice but to fight on the battleground imposed on them" (p. 27).

Roger H. Davidson examines the charge that the introduction of the abortion issue has diverted congressional attention from pressing socioeconomic problems and is threatening to undermine norms of political civility and respect for "rules of the game." Davidson finds that surrisingly little time has been devoted to hearings and debate on abortion and argues that, while both sides have acted in ways that were not completely fair, "the tactics that have been employed are well within the bounds of the usual practices and so-called rules of the game" (p. 85).

G. Calvin Mackenzie examines the use of abortion attitudes as "A Test of Fitness for Presidential Appointment." Questions on abortion began to be raised during the Carter administration, but it was not until the Republicans took control of the Senate that some legislators declared their intention to judge nominees solely on their position on abortion. Mackenzie is concerned about this trend, but believes that guidelines can be established that recognize the legitimacy of the abortion issue but that would give it heavy weight only for those positions directly responsible for abortion policy. Since most abortion policies "are about as unambiguous as a policy can be" (p. 60), leaving little latitude to those administrators interpreting the law, Mackenzie is hopeful that as both sides come to understand the limited opportunities the appointment process affords them in shaping policy, "the number of battles that will be fought on this ground can be limited to those few that hold out some hope of a meaningful victory" (p. 63).

The final essay by John Mackson and Maris Vinovski assesses the effect of single issue pro- and antiabortion groups in the electoral process. They argue that most voters have mixed views on abortion, and that relatively few place heavy weight on a candidate's abortion views when casting their vote. The success of pro-life and pro-choice forces depends largely on the fortunes of the larger political coalitions of which they are a part. Pro-choice groups lost strength in 1978 and 1980 "by virtue of their identification with the liberal agenda and with the Carter administration" (p. 80). While the antiabortionists gained influence as a result of the Reagan victory, their gains have been limited. Jackson and Vinovski contend that "pro-life groups have not been able to impart their previous sense of urgency since adoption of the Hyde amendment and inclusion of pro-life partisans in the Reagan administration. . . . Paradoxical as it may appear, partial victories seem to strengthen the losers while simultaneously weakening the winners" (p. 80).

The essays in this volume offer clear, well-written analyses of the impact of the abortion controversy on the political system. All the contributors accepted the legitimacy of the abortion issue and were scrupulously evenhanded in discussing the activities of the two sides. No better overview of the topic is available.

JAMES DAVID FAIRBANKS

University of Houston-Downtown

The Politics of Public Utility Regulation. By William T. Gormley, Jr. (Pittsburgh: University of Pittsburgh Press, 1983. Pp. xi + 271. $26.95, cloth; $9.95, paper.)

The title of Gormley's book implies two areas of interest, regulation and energy. The content also includes, with almost equal prominence, the politics of public participation, state policy determinants analysis, and interest group theory. The unifying element is an effort to sort out the roles of varied policy influencers—public utility commissioners, commission staffs, and utility firms—and two types of public advocates—proxy and grass roots. Proxy advocates are officials (state attorneys general offices or consumers' counsels) charged with representing rate-payer interests; grass-roots advocates include consumer, antinuclear, environmental, and low-income groups.

Within the scope indicated, Gormley has given us a highly competent study of a policy arena of great current political salience, one that is also enjoying something of a scholarly renaissance. Data published in recent journal articles is incorporated into the book, but accounts for less than one-half of it. The articles focus more directly on an attempt to measure public advocacy in utility commission proceedings. The book places that material in the broader framework, topically and conceptually, of the totality of utility regulation as a political phenomenon. In the process, the data reported earlier have enhanced meaning, and Gormley has the intellectual and physical space needed to develop his policy-relevant interpretations, plus some advocacy of regulatory reform proposals aimed toward simultaneous enhancement of the values of democratization and professionalism.

Substantial funding from the National Science Foundation enabled Gormley to develop a strong information base, from which he presents data that will prove helpful to those who share one or more of his interests. His book draws on responses to a questionnaire sent to 188 state utility commissioners in all 50 states, interviews with 284 respondents representing each of the posited categories of influential actors, and several hundred utility commission documents. The documents and interviews were concentrated in 12 states, three each representing the four categories of Gormley's public participation focus: grassroots emphasis, proxy emphasis, high levels of both, low levels of both. Appendixes explain the approach and include the questionnaire.

Drawing on an unusually wide range of literature for relevant hypotheses and comparisons, Gormley is able to comment on a number of frequently discussed analytic issues regarding formative factors in public policy analysis. The "capture theory" proposition, that the regulatees capture the regulators, is supported in his findings only to the extent that utility companies are perceived to "wield more power than any other outside participants" (p. 140). The cumulative weight of proxy and grass-roots advocates "may be considerable," however (pp. 140-141). In three states, proxy advocates were actually assessed as more influential, or nearly so, than utilities. As James Q. Wilson suggests in his writings, Gormley finds that capture is least likely when other interests are represented explicitly, hence the advocacy groups and proxies do have policy significance. However, perhaps a more basic finding is that the cleavages are issue-dependent and so diverse and complex as to belie such simple categorizations as conservative or liberal. They include not only consumers and utilities, but also residential versus business consumers, middle-class rate payers and those who seek to aid the poor, and conservationists whose concerns are sometimes less for rates than for encouragement of alternative approaches to energy.

In the end, Gormley synthesizes his material into what strikes me as a useful framework of complexity and "conflictuality" as the distinctive variables in the politics of public utility regulation. He then relates these to the posited goals of professionalism and democratization and describes the crucial dilemma (p. 217):

> When issues are complex, participation without expertise can be useless; when issues are conflictual, professionalism without accountability can be manipulative; when issues are complex and conflictual, there is a special need for institutions that promote expertise and accountability simultaneously.

While I would wish that this were a longer book so that Gormley could have developed further the relationship of his data to the burgeoning propositions of public policy theory, perhaps he will do that in future writings. Meanwhile, we have a major addition to the disciplinary literatures of regulation and public participation and a substantial contribution to our understanding of how things operate in energy and communications regulation.

MICHAEL D. REAGAN

University of California, Riverside

The President and the Public. Edited by Doris A. Graber. (Philadelphia: Institute for the Study of Human Issues, 1982. Pp. v + 310. $17.50, cloth; $7.95, paper.)

One problem with democratic elections is that the people may not have a choice between candidates that they would most prefer. Another problem is that in the twentieth century, media shapes the information people receive about the candidates, which could hinder the free choice of candidates. Doris Graber's edited book deals with these problems.

What do people want in a president? Are they able to get the kind of person they would like? In the second chapter, based on 1979 Gallup poll data, Stephen J. Wayne finds that the people want a strong president before any policy concerns. This finding fits well with Survey Research Center data that show that the public viewed Reagan as a potentially strong president although they disagreed with him on most policy issues. In addition, data in the chapter by Lee Sigelman indicate that, contrary to all the evidence of a dramatic decline in public confidence in government, people are satisfied with the government. Sigelman argues that there has been no recent crisis of confidence when one looks at other poll findings as well as data from the 1930s. In contrast, in his chapter on the electoral college, Lawrence D. Longley argues that the way Americans elect a president works against the one-person-one-vote ideal democratic rule. And, the commonly viewed assumption that the electoral college enhances the power of blacks is wholly untrue and actually works against this subgroup's voting strength.

The Gallup poll presidential rating question is the most commonly cited indicator of the public satisfaction with the person who leads them. Studies have shown that every modern president experiences a decline in support after inauguration, and that foreign "rally around the flag" events increase a president's support. Cliff Zukin and J. Robert Carter's fine chapter on using such Gallup data makes one think twice about taking the data at face value. They raise problems with, among other things, the Gallup's wording of the question so our interpretations of the public's satisfaction with the president could be distorted. While their Eagleton poll data do not really support their methodological points, Zukin and Carter's chapter is important reading for all scholars who deal with presidential public ratings.

Another of the book's themes is the issue of media distortion of political information. A special case of media influence can be made for the coverage of a president seeking reelection. Once in office, no one can please all groups, and, as President Reagan has reminded us, bad news is good news for the media. A chapter by Martha Joynt Kumar and Michael Baruch Grossman in the "Media Portrayals" section presents a content analysis of *The New York Times* and *Time* between 1953 and 1978 and the CBS evening news from 1968 to 1978. Two coders classified White House stories into positive, negative, and neutral categories as well as by subject of story, such as personal or policy. Kumar and Grossman found that the number of negative stories have increased, although they still are outweighed by positive stories. The extent to which these stories shape public opinion is left unexplored. It would have been helpful to have seen Gallup poll data rating the presidents on the same chart depicting the percent negative stories over time.

William C. Adams also speaks to the media influence issue when he makes the case that in every election since 1960, excepting 1980 and 1972, one candidate had the edge in favorable television coverage while the other had the edge in favorable newspaper coverage. Barry Goldwater was the only television favorite to lose the election, but he was also the only losing candidate with the television advantage to run against an incumbent president.

I recommend this edited book not as an integrative, major work, but rather as a collection of recent research on the president and the public, useful for political scientists and graduate students.

ALANA NORTHROP

California State University, Fullerton

Influence, Change, and the Legislative Process. By Janet Miller Grenzke. (Westport, Conn.: Greenwood Press, 1982. Pp. xi + 193. $35.00.)

The central question of this volume is whether state legislators change their attitudes and behavior while in office. This concern has become a widely discussed and interesting new direction in the study of representation and legislative politics. Grenzke argues convincingly that we must begin to study incumbent change in state legislatures because turnover rates in most states are decreasing. For a legislature to be democratic, its members must be responsive to the wishes and the interests of the people; they must alter their beliefs and behavior to conform with the wishes of their constituents. Grenzke is not interested in the type of representation exhibited as response to individualistic services such as baby books and social security checks. Issue responsiveness is the key to representation, since such responsiveness is the mission of a legislature in its role as a democratic institution. At the end of the extensive analysis, Grenzke concludes, "At the state level, democracy is not a charade" (p. 158).

This work is based on a 10-year longitudinal study of the Michigan House of Representatives. The research design for the study entailed an extensive amount of personal interviews, content analysis of legislation, and voting analysis. As always, one can quibble with the selection of issue areas, the way in which bills were selected, and with some of the indicators; however, for a work which requires operationalization of so many concepts, the degree to which Grenzke is able to measure what she intends to measure is quite impressive and separates this book from others that attempt to include a variety of indicators of determinants of legislative behavior. At the same time, she does fill in the substantive details of her cases in order to flesh out the explanation.

The book is concerned with three policy areas: education, business, and racial policies (a fourth, the budget, emerges in some of the attitudinal analysis). The issue domains were selected because of their conflictual nature and lasting salience as well as for their differences as political bargaining arenas. Grenzke finds that in two of the areas, race and education, the leglislature was quite responsive. Both issue domains are highly visible ones on the state level, and the result is not unexpected. That the business domain, and presumably most of the less visible issue areas (retirement, criminal procedures, health and safety, local and municipal affairs) that were not selected because of their lack of prominence, are not as responsive suggests that Grenzke's enthusiasm over the state of democracy in Michigan is perhaps overstated.

Of her many insights, perhaps the most fascinating is the notion that incumbents change their information gathering patterns of interaction over time, and that it is incumbent change and not just turnover that makes legislative representation so viable. She finds that especially during the civil rights crisis, legislative incumbents led the way in responding to their constituents, and low turnover legislatures could be just as responsive as legislative sessions marked by high turnover.

Influence, Change, and the Legislative Process reads almost like a general legislative politics textbook in that it draws from every part of the literature. Those who are familiar with the standard works on legislative politics will find the book highly integrative; those who are not might be overwhelmed by the degree to which several disparate theses are woven together into one set of hypotheses and conclusions. While the results of the analysis are not striking, the book is a welcome, well-executed addition to our understanding of the nature of representation in the legislatures.

T. Wayne Parent

Louisiana State University

Crime in City Politics. Edited by Anne Heinz, Herbert Jacob, and Robert L. Lineberry. (New York: Longman, 1983. Pp. xii + 288. $25.00, cloth; $10.50, paper.)

Crime in City Politics is part of a larger study of crime, criminal justice, and urban politics in 10 cities that Anne Heinz, Herbert Jacob, and Robert Lineberry started in 1978. Looking at governmental responses to crime from 1948 to 1978, the editors supervised a vast research project that yielded a tremendous amount of data on city residents, newspaper coverage of crime, the character and substance of city policy, and interviews with public and private authorities. Three technical reports already published by the National Institute of Justice offer data summaries and analyses, while future monographs promise more expansive consideration of the "crime-politics-policy connection" (p. xi).

Crime in City Politics offers another cut from this research project, but with a decidedly distinctive perspective. The editors offer summary essays on crime, politics, and urban affairs, and provide five case studies gleaned from the 10-city project. The anthology consists of an introductory essay and conclusion by Jacob and Lineberry and case studies by the site directors in five of the 10 cities. The case studies offer detailed, contextual analyses of the relationship between crime policy and city politics. Together with the introductory and concluding essays, they are welcome additions to the study of urban politics, criminal justice, policy analysis, and state and local government.

While *Crime in City Politics* is instructive because it offers the kind of case study not prominent in many political science journals, its substantive merit rests on the lessons that the editors draw from the case studies and on the way these analyses illustrate some established points about crime and our efforts to deal with it. In the introductory and concluding essays, Jacob and Lineberry argue that there is a deep and longstanding connection between criminal justice policy and city politics. They emphasize that this relationship predates the heralded, national politicalization of crime in the 1960s. In addition, Jacob and Lineberry point out that governmental responses to crime haven't changed much—in fact, there is not much variation from city to city. While they do not pretend to have examined governmental responses to crime in every major American city, their in-depth study of 10 major and distinctive cities provides persuasive evidence. To be sure, the statistical evidence is largely aggregate and the contextual documentation somewhat interpretative. But the similarity in patterns of city policy and the communalities observed make for a compelling argument and demonstrate the importance of a variety of research strategies in political science.

In each of the five case studies and in the initial and concluding essays, several substantive points about criminal justice policy and the problem of crime are illustrated or emphasized. The studies of Newark, Philadelphia, Minneapolis, Phoenix, and San Jose demonstrate several things: that police expenditures don't have much of an effect on crime, that crime rates can be and sometimes are manipulated by local police and political authorities, that the fear of crime is frequently grounded in apprehension about its unpredictability and not the actual probability of victimization, that crime is related to other social problems, that the political debate on crime is often couched in symbolic terms, that police dominated the early LEAA grant process, that the expenditure of federal funds did have some positive effects on criminal justice if not on the problem of crime, that there is a close relationship between city hall and the police, and that the police are more professionalized than in earlier eras.

Although the authors of the five case studies and the editors have done a fine job of providing the rich contextual analysis so central to the larger research project, the anthology does have some limitations. Substantively, some connections should have been drawn with related studies that focused on the same cities. Additionally, it would have made for a more coherent collection if the authors of the five case studies had explicitly focused on the themes that Jacob and Lineberry drew at the beginning and had organized their respective chapters around them. Finally, an index would be helpful.

The merits of the collection, however, far outweigh the shortcomings. The anthology provides a good illustration of contextual political analysis. As such, it demonstrates the range of research strategies that can be applied with considerable benefit to our understanding of politics. It also demonstrates how closely crime and criminal justice are tied to the political arena. More compelling, perhaps, than these strengths is an assumption implicit throughout *Crime in City Politics.* The editors and authors suggest that crime is a problem that can't be solved. While they do not explicitly focus on the limitations of criminal law, they implicitly direct the reader's attention to the social, legal, and political institutions that share responsibility for society's moral and social problems. In this fashion, the book serves as a useful resource, not only for students of criminal justice but also for the contemporary politicians who persist in promising easy solutions to the vexatious problem of crime.

SUSETTE M. TALARICO

University of Georgia

The Hardest Drug: Heroin and Public Policy. By John Kaplan. (Chicago: University of Chicago Press, 1983. Pp. x + 247. $20.00.)

John Kaplan is concerned with what the criminal justice system should do about the problem of heroin in our society. Finding major flaws in the present national policy of total prohibition, Kaplan examines the costs and benefits of alternative policies toward heroin. His objectives seem overly ambitious: "to solve the particular social problem" and "to get those in power to undertake appropriate remedies" (p. 2). For Kaplan, the preferred solution involves requiring treatment programs for heroin addicts and ending reliance on the criminal law to control the behavior of otherwise law-abiding drug users.

Much of the argument in this provocative volume is based on Kaplan's perception of heroin as a social problem. In his view, policymakers make decisions on false assumptions; for example, users of heroin inevitably become addicted, "once an addict, always an addict," and addiction is a disease. Heroin addiction, says Kaplan, is nothing more than a "bad habit" much like alcohol consumption or cigarette smoking. Addicts use heroin, for the most part, "because they like it" (p. 49). What troubles Kaplan most, however, is the assumpion that most heroin users commit property crimes. Although he makes too much of inconclusive data, Kaplan may be right when he asserts that "an increasing percentage" of contemporary heroin users are otherwise socially responsible and law-abiding. A beneficial and humane policy, therefore, would differentiate between casual users and addicts who commit crimes to support their habits.

Current national policy, of course, does not make a distinction between the noncriminal user and the addicted criminal. It is based on the Harrison Act of 1914, which made illegal the sale, importation, and possession of opiates in the United States. Kaplan maintains that this approach has produced many unacceptable consequences ("costs") including an increase in crime, the creation of a black market in drugs, an erosion of civil liberties, overcrowded dockets and prisons, and pain suffered by addicts, users, and their families. In Kaplan's view, these "costs" outweigh any benefits that might be gained by using the criminal justice apparatus against both addicts and noncriminal users of heroin.

Although Kaplan is skillful in his use of interdisciplinary materials, there are several methodological problems. We are told, for example, that the costs and benefits of alternative policies will be examined. However, no rigorous cost-benefit analysis is undertaken. Rather, Kaplan tends to focus on the advantages and disadvantages of each policy. Nor are we offered a statement of the criteria to be used in evaluating the alternatives. Also, even Kaplan concedes that much of his analysis is made on the basis of insufficient data.

Kaplan's solution to the heroin problem is presented in the last chapter. He argues that a program of "coerced treatment" using methadone would be more economical and effective than imprisonment for the use offender. Under this program, the violator of a heroin offense would either be required to enter a treatment center as a condition of probation or be imprisoned. There is some evidence, moreover, that "coerced treatment" would reduce both the amount of heroin use and the crime associated with use. Given the high costs of isolation and deterrence, Kaplan recommends extending this program to addict criminals. Crimes committed by nonaddicted users would not be handled under the treatment program, since there is no causal connection between drug use and criminality. Finally, Kaplan argues that a cost-effective and humane policy would remove all governmental intrusion into the lives of law-abiding nonaddicted users of heroin.

In the final analysis, Kaplan's solution to the heroin problem is not convincing. He concedes, for example, that "we cannot be sure whether we would be better off ignoring all use offenses and coercing into treatment only those addicts who have been caught committing crimes" (p. 233). Also, as Kaplan admits, no attention has been given to the political obstacles that might prevent adoption of his proposal.

RODNEY A. GRUNES

Southwestern At Memphis

State Government Influence in U.S. International Economic Policy. By John M. Kline. (Lexington, Mass.: D.C. Heath, 1983. Pp. xiii + 266. $27.95.)

During the past decade, state governments probably have become more influential in domestic issues. A main assertion of this book is that state governments also have emerged as a significant force in affecting U.S. international economic policies. Despite our recognition that the U.S. is an intergovernmental structure, this premise is sure to provoke skepticism.

Assume momentarily that the assertion is correct. Why have states become important actors in international economic issues? According to Kline, two primary changes have occurred. First, global economic interdependence has penetrated to a level where state government interests are affected and where states possess significant legal or regulatory authority. Second, state governments have more organizational capabilities within their states and on an interstate basis, they have improved their lobbying campaigns, and they have become more successful in achieving their aims. Put differently, international economic forces have "provided the states with the economic interest and, through overlapping jurisdictional authorities, the operational levers, to become involved . . . [while] concomitant organizational improvements in state lobbying and support mechanisms provided the access channels to project state influence to the national policymaking level" (p. 3).

Overall, the major arguments and the case study data are convincing, with several notable exceptions. The most glaring deficiency is the absence of an answer, or a model for deriving an answer, to the question of how influential have states become. Are states now more influential on most issues than international banks or the State Department? Kline does not claim states now play a dominant role, but readers are left with little idea of the relative influence of states in comparison with other actors across a range of economic issues.

A second major weakness is the evidence presented on the organizational capability argument. States are more organized than they were a decade ago, but so are all the other players. While the case studies present instances where success has been achieved, most state government leaders still subscribe to the "helpless executive thesis" on their ability to control matters within their own states. Governors as a group have even less influence on national economic issues and on international economic policies; most governors would see their influence as infinitesimal despite a stronger lobbying presence in Washington. State legislators are practically ignored as a relevant group, an omission which would be hard to defend in the numerous states where the legislature is at least as strong as the governor.

There are other problems with the study. It is too long and the intended audience is unclear. Few timely recommendations are made, so the book would be marginal for practitioners. Although more appropriate for the academic community, the focus on states will inhibit its attractiveness to international relations scholars and researchers. A final problem is one of perspective. While there has been growth of state trade and investment promotion activities over the past 15 years as shown by Kline's data, on a relative basis the percent of state budgets and state economic development budgets allocated to foreign promotion in all cases is very small—less than 5% of states' economic development budgets. Even as economic development has been building toward a crescendo on states' agendas, and as some states are establishing specific export trade financing vehicles, state governments simply are not devoting many resources to international economic matters.

For all its limitations, this book has identified and documented the presence of a new group of actors in some international economic issues. In addition, there are excellent cases presented on states' investment restrictions, the unitary tax approach, and suggestions for redefining appropriate roles for states and the relevant federal agencies.

Kline believes his research is "field defining" rather than "field refining." It is. Yet it is also essentially a book on the power of state governments. Unfortunately, that brings all the attendant problems that empirical power research has had over the past 25 years. Some readers will be disturbed by the imprecise methodology and by the imprecise conclusion that states are more important than they were previously; others will overlook this problem and view the study as a pioneering one.

JAMES E. JARRETT

Texas Advisory Commission on Intergovernmental Relations

Environmental Regulation: The Political Effects of Implementation. By Laura M. Lake. (New York: Praeger Publishers, 1982. Pp. 153. $20.95.)

This is an overly ambitious work that covers a plethora of environmental law cases and political science theories. Although primarily useful as a bibliographical reference, it does present some interesting ideas, for example, that congressional exemption from environmental regulations of major energy projects such as the Alaska Pipeline has violated the "legal rights of due process and state sovereignty" (p. 22), and that the increasing use of the courts for environmental litigation is undermining the judiciary's legitimacy and emasculating the power of Congress. Unfortunately, the ideas are not systematically developed within a theoretical framework. There are even the beginnings of some worthwhile case studies in this work that are not adequately developed.

The thesis of *Environmental Regulation* is that "in the process of attempting to implement federal laws, political relations and institutions are changed in profound ways" (p. 1). This is particularly true of post-1969 environmental protection and pollution control laws, which mandated specific standards and deadlines and granted citizens the right to sue the national government and state governments to force compliance. Hence, state sovereignty is undermined, and citizens and public interest groups increasingly engage in litigation rather than in the lobbying of legislators.

We are told that the flood of environmental legislation came about because presidential contenders such as Senators Muskie and Jackson and President Nixon were eager to be identified with environmental protection. However, Lake ignores, for example, that Edmund Muskie had a pro-environmental record that dated back to his governorship of Maine in the fifties. She also neglects the implications of partisanship. Riley Dunlap, who is cited in Lake's excellent *Political Science Quarterly* article on mass environmental attitudes (1983, *98,* 215-233), has demonstrated through roll call analysis that Democrats are significantly more supportive than Republicans of environmental proposals at both the congressional and legislative levels just as they are generally more supportive of regulatory proposals.

A peculiar aspect of the work is Lake's criticism of environmentalists for their exploitation of the judiciary because they perceive its members to be more sympathetic to environmental goals than members of the executive or legislative branches. Yet, she admits that legislative monitoring of environmental regulation is inadequate and that the Carter and Reagan administrations have been insensitive to environmental goals. One reason she considers environmental reliance on the courts a mistake is that surveys have shown members of the bench to be economic conservatives, and hence, if they acquire the habit of engaging in substantive judicial review in environmental cases, they may also do so in economic ones, but in those cases decide in a conservative manner. Why should that concern an environmentalist? It is well known that ideological constraint is low in the United States. Additionally, Lake finds in her *Political Science Quarterly* article that environmentalists are mainly centrists and so not necessarily liberal on economic issues. She is concerned that in the long run the legitimacy of the judiciary itself, not to mention that of the other branches of government, will be undermined. It seems unrealistic to expect environmentalists with short-range political goals in mind to be concerned about the legitimacy of the judiciary.

Environmental Regulation makes only a modest contribution to our knowledge of the political effects of implementation. One hopes that Lake more fully develops a selected few of its themes in later writings.

HENRY B. SIRGO

McNeese State University

The Reluctant Patron: The United States Government and the Arts, 1943-1965. By Gary O. Larson. (Philadelphia: University of Pennsylvania Press, 1983. Pp. xvi + 314. $30.00, cloth; $12.95, paper.)

The Reluctant Patron is an account of the years between the demise of the New Deal art programs and the establishment of the National Endowments for the Arts and Humanities during the Great Society era. Contrary to the common perception that little happened regarding arts policy during this period, Larson demonstrates that a great deal transpired, and that it was related both to what preceeded and to what followed. Essentially, *The Reluctant Patron* implies that the policies of the New Deal era failed to establish the arts as an item on the federal policy agenda. It then proceeds to recount how arts policy reappeared on the public agenda, as well as what attitudes, activities, and events influenced the eventual formulation and enactment of a new federal arts policy in the mid-1960s.

Larson is best at detailing the long debate over a public role in the arts. The arguments that developed during this period, both for and against governmental involvement, clearly resound into the present. The chief objections as voiced then (pp. 78-92) are amazingly similar to the initial positions of the Reagan administration. These included the belief that federal involvement in the arts represented an "invasion" of the proper authority of the states, that it would result in centralized censorship and encourage mediocrity rather than excellence, that there were more urgent and important ways to spend federal money (particularly when confronted by large federal deficits and an increasing public debt), and, finally, that the arts were historically an area where private initiative and local support were paramount.

Similarly, proponents of a supportive federal policy toward the arts, both then and now, argue that the arts are an element of national prestige, are a useful tool of cultural diplomacy and enhance the quality of life domestically. Furthermore, they observe that the precarious economic condition of the arts seems to require an infusion of public funds if they are to continue to flourish. (See chaps. 6 and 7.)

This book is well-reasoned and well-documented. Larson has made extensive use of primary sources, including both government documents and individual interviews. Indeed, he presents a very useful guide and summary of many of the relevant congressional hearings, public documents, and arts community activities of the period.

Despite its merits, however, *The Reluctant Patron* is flawed and incomplete as an exercise in policy analysis. In many ways the book is too narrow. For example, the events of the period are seen primarily from the perspective of the visual arts. Relatively little is said about the performing arts. While there are similarities between the two, there are also important differences in the course of governmental-artistic relations in each area. Even within the visual arts, Larson overemphasizes the importance and political impact of abstract expressionism. Although probably the most dynamic artistic movement of the late 1940s and the 1950s, surely it was not the only artistic activity exerting an influence upon public, especially congressional, opinion regarding the arts.

The book focuses, perhaps too heavily, on congressional actors, attitudes, proposals, and actions. The changing social and economic context and its impact on the arts is not fully articulated. Although alluded to briefly, the appearance and relevance of various state or foreign bureaucratic models such as the New York State Arts Council or the British and Canadian arts councils are neither described nor discussed adequately. Finally, Larson presumes that his readers are intimately familiar with key events and actors in this policy area. As a result, his narrative is occasionally cryptic, and his sense of time is sometimes vague and uncertain.

In short, *The Reluctant Patron* is an excellent effort to gather and present significant information about what happened regarding federal arts policy from 1943 to 1965. It fails, however, to explain why these developments occurred. Thus, instead of analyzing the roots of current public policy on the arts, *The Reluctant Patron* merely collects, summarizes, and distills much of the raw material that will be necessary to undertake such an analysis.

MARGARET JANE WYSZOMIRSKI

Rutgers University

Urban Land Policy for the 1980s: The Message for State and Local Government. Edited by George Lefcoe. (Lexington, Mass.: D.C. Heath, 1983. Pp. xix + 211. $28.95.)

This book is a compendium of 10 papers, with comments and discussion, presented at a conference, sponsored by the Lincoln Institute of Land Policy, to assess the implications of the Reagan new federalism for state-local urban land policy. The papers are uniformly excellent, typically presenting the latest research of the mostly academic authors. The central thread is the role of urban land as a public resource.

The compendium is organized into six topical sections. In section 1, Larry Kimbell and David Shulman's paper forecasts the implications of Reaganomics for California governments. They anticipate a one-third drop in public infrastructure investment. In 1982, California lost $1.5 billion in federal funds. While tax relief should spur business growth, this growth will demand infrastructure investment limited by tax ceilings. In section 2, Donald Hicks (author of the Carter administration's urban policy report) argues that the purpose of a national urban land policy should *not* be to channel urbanization into declining areas or away from growing areas. Continuing suburbanization results from basic economic trends generated by irreversible evolution from an industrial to a service economy.

Section 3 examines the British national land planning system. The U.K. has a very different approach to housing, land use, property taxation, and financing of local services involving a large public housing sector and major central government subventions. Peter Hall concludes that British policies established in the 1947 Town and Country Planning Act, which nationalized the right to develop land and provided compensation for private losses, have been significantly reversed by the Thatcher government. The new approach —involving efforts to reduce spending growth by local authorities, accelerate sale of public housing to tenants, and develop a private building industry—would have been forced by economic conditions regardless of party. Donald Denman criticizes British policies as lacking a defined mission. The inevitable result has been ambiguity and conflict over land use decisions. He draws the basic lesson that public land planning will necessarily fail unless private incentives coincide with well-defined public goals.

In the next section, Dale Keyes evaluates the Reagan energy and environmental programs, with interesting comments by Richard D. Lamm, governor of Colorado. Keyes concludes that rising fuel prices will *not* reshape location preferences. Better insulation, smaller homes, and fuel-efficient cars should permit continued suburbanization. He foresees conflict between polluting and amenity-conscious industries over suburban locations. Lamm is critical of changing federal policies that might endanger the environment. The federal government has an enormous influence over land use, economic development, and environmental quality in the Western states.

The fifth section focuses on state-local financing with papers by George Peterson on public infrastructure, E. Gareth Hoachlander on education, and Patrick Coughlan on innovative alternatives. Capital spending has dropped to one-half its share of state-local budgets since 1960. However, federal standards may bear no necessary relationship to minimum-service levels. School finance in California has declined by 15% since Proposition 13, but federal aid is only 2 or 3% of district budgets. Hoachlander recommends tuition tax credits, vouchers, and smaller districts to promote community participation. State assumption of school financing frees district structure from the local property tax base. Coughlan evaluates user charges, special assessments, and special taxes; California governments are particularly looking to developers and businesses.

The final section concerns national housing policy. Wallace Smith recommends replacement of the thrift industry by a government lender. The industry has collapsed trying to compete for non-mortgage financial services. Ira Lowry finds a surplus, not a shortage, of rental housing measured in terms of returns to investors, who are going bankrupt from rising real costs and falling real rents. He recommends rent supplements for the truly needy rather than rent control or construction subsidy.

DUANE WINDSOR

Rice University

Wall-to-Wall America: A Cultural History of Post-Office Murals in the Great Depression. By Karal Ann Marling. (Minneapolis: University of Minnesota Press, 1982. Pp. xiv + 348. $35.00, cloth; $14.95, paper.)

If *Wall-to-Wall America* were only the careful study that it is of the mural art commissioned by the Treasury Department's Section on Fine Arts during the New Deal, it would deserve the attention of students of American politics. Between 1933 and 1942 about 1,200 murals were competitively selected to embellish post offices around the country. The mural program was part of a much larger federal cultural effort and is not to be confused with the better-known WPA arts projects. The WPA existed primarily to give work to unemployed artists. The Section, however, sought foremost to produce art of good quality that would both satisfy and elevate popular taste and would be suitable for a public facility. This was no easy set of goals and that many murals were highly controversial should not be surprising.

Marling's book, however, recommends itself for more than its evaluation of the artistic properties of the murals and of the Section's administrative history. *Wall-to-Wall America* provides an analysis of the political and ideological contexts in which decisions were made about which murals to commission. Politically, mural selection involved the interaction of an "iron triangle" of patron, painter, and public. As patrons the Section's cultural bureaucrats were painstaking in their scrutiny of mural production from design to execution. They were particularly vigilant to safeguard against any thematic or stylistic deviations from what they deemed to be high quality. Artists had wide freedom in executing murals on acceptable themes. Despite a tendency to "paint Section," murals encompassed a fair variety of artistic styles although the juried selection process favored the conventional. The public could be understanding on stylistic matters, but it was insistent that the murals be accurate in depicting local geography, traditional in the depiction of historical incidents, and unconcerned with the harsher realities of the Great Depression.

This description of the process of cultural policymaking is fascinating, but more illuminating is what this public art says about the intersection of art and ideology. Marling argues that the subject matter and to a lesser degree the artistic style of the murals (such as the avoidance of modernism) tells us what Depression-era Americans thought about themselves, their present, and the past. Marling describes the iconography of "murals as murals should be" accordingly:

> Work was the fiber of the past and the key to the future; it was the steady, business-as-usual heartbeat of Mural America. Grim or sad expressions, even pleasant ones, are hard to find there. The visages of the American workers are, at their emotional extreme, merely preoccupied; they hold their breath at the thrill of a job. Most of the time, they are sublimely contented and content to let their faces register the homely, humdrum routine of a welcome day's work, well and truly done. (p. 174)

Murals that deviated from the accepted ideology risked rejection, revision, ridicule, or (as happened in Aiken, South Carolina) being hidden from public view.

Wall-to-Wall America crosses the boundaries of history, art history, and political science. These largely forgotten and poorly judged post office murals could become an essential element for our understanding of popular attitudes in the 1930s. It is an impressive undertaking in interdisciplinary conceptualization; equally impressive is the archival and field research that was involved. While this book will be of greatest interest to those who specialize in cultural policymaking, it should also be required reading for field seminars in American politics. The question of whether these works are good or bad art is largely incidental to their political value for what they tell us about the style of mass belief systems.

KEVIN V. MULCAHY

Louisiana State University

Political Parties and the Development of Black Insurgency 1930-1970. By Doug McAdam. (Chicago: University of Chicago Press, 1982. Pp. viii + 304. $25.00.)

Doug McAdam provides strikingly logical reviews and analyses of the "classical model" of mass movemnts (variously titled relative deprivation, rising expectations, status inconsistency) and the resource mobilization model and proposes a third, the political process model. Unfortunately, that ambitious and impressive beginning stalls midway through the book as McAdam compares various aspects of the three models to the civil rights movement.

First, McAdam seeks to unite the political science and sociological literatures which, he states, have developed in isolation of each other. Second, he rejects the assumptions of the classical model, that the significant actor in mass movements is the isolated anomic individual in search of social-psychological security, and that social strain is the primary variable which gives rise to mass movements. He disassembles the components of the models grouped under the broad title classical theory and hypothesizes that a mass movement is likely to occur when variations arise in factors other than social strain.

Third, McAdam argues that the resource mobilization model focuses on the role of elites in providing dissatisfied groups with the means to challenge the status quo but points out the logical fallacy involved; in all probability elites seek to contain mass movements rather than to goad them into political conflict. McAdam notes that even disadvantaged groups experiencing social strain have resources that can be mobilized effectively under certain circumstances into an organized political movement.

McAdam's political process model is thus based on the assumption that social or political movements arise from variations in indigenous organizational strength, the structure of political opportunities in the larger community, and the degree of cognitive liberation in the group. The model identifies and successfully resolves a number of anomalies left unsettled by the social strain theory through McAdam's excellent analysis.

Finally, McAdam examines the context for the rise of black insurgency, and its generation, peak, and decline. Here I found his treatment of the historical context of the civil rights movement most convincing, with the successive chapters decreasingly satisfactory. The empirical sections of the book are weak on two counts. Most of the data, on demonstrations, organizations, and issue areas are based on *The New York Times Index.* It strains credulity to think that the *Index* is more than a rough indication of events; some types of protest and some protesting organizations may have been underreported relative to others. Since McAdam rightly notes it is difficult (although I would argue not impossible) to find alternative sources, I now turn to his inconsistent use of data.

In one example, McAdam identifies several types of groups: churches, colleges, and formal movement organizations. He also creates a separate category for local NAACP chapters, but places the *national* NAACP office with formal movements without explanation (p. 126). Next, McAdam provides no definition of insurgency. Political protest, demonstrations, and marches might, from the text, seem to be a good working definition, except that the NAACP's legal actions are also counted as insurgent. Since the NAACP has been active in legal areas for decades, should its earlier activity be considered insurgent? If so, what provoked it? These are important issues because they strain McAdam's attempts at creating a theoretically consistent model.

Finally, McAdam presents two tables which summarize movement initiated events from 1955 to 1960 and from 1955 to 1965. In the first, he argues that black colleges, churches, and the NAACP initiated the majority of movement events from 1955 to 1960. In the second, he argues that formal movement organizations dominated the initiation of events from 1961 to 1965. The number of movement incidents in the 1955 to 1960 period doubles without explanation from the first table to the second (pp. 126, 147). Secondly, what does it mean for his model if McAdam emphasizes the shift from indigenous (presumably church, college, and NAACP), to formal organizations over time, but the NAACP has been categorized as *both* indigenous and formal? Since the NAACP disappears from the second table even in local form, I presume McAdam has converted it into an entirely formal group, thereby making his point through, at best, confusing procedures.

These and other data application and interpretation problems are particularly unfortunate. Despite my criticism of his handling of the civil rights movement, McAdam's theoretical analysis and his political process model disentangle a great many previously unsatisfactory explanations of these events.

DIANNE M. PINDERHUGHES

Dartmouth College

Fiscal Federalism and the Taxation of Natural Resources. Edited by Charles E. McLure, Jr. and Peter Mieszkowski. (Lexington, Mass.: D.C. Heath, 1983. Pp. v + 260. $35.95.)

The chapters of this book were presented originally as papers at a conference of the Committee on Taxation, Resources and Economic Development (TRED) under the auspices of the Lincoln Institute of Land Policy. The majority of the authors are economists by profession, but the issues addressed make this book of considerable appeal and usefulness to political scientists who are resource and fiscal policy specialists as well as those concerned with intergovernmental relations and constitutional law.

Two major themes recur throughout the book. One basic set of issues concerns the legal and constitutional aspects of tax jurisdiction in the federal systems of the United States and Canada where natural resources, particularly energy resources, are involved. The provincial governments of Canada have broad and explicit constitutional powers to tax as well as to control resource development and revenue stemming from ownership of crown land and from their ability to play an entrepreneurial role in the production and marketing of provincial resources. By contrast, the states have more limited powers and have been subjected to litigation and congressional scrutiny regarding the rate of severance taxes levied by energy producing states. Constitutional provisions contained in the commerce clause and in the due-process and equal-protection clauses of the Fourteenth Amendment have proved to be insufficient bases for Supreme Court adjudication of tax controversies, thus placing the major responsibility for federal intervention in state taxation policies with the Congress.

The other major focus of the book is upon the disparities among the states and provinces in fiscal capacity. Because of the dramatic increase in energy prices following the actions of the OPEC cartel, states with substantial coal and oil deposits have had available unprecedented revenue sources flowing from energy production. Severance taxes, royalties, property taxes, and corporate income taxes are some of the mechanisms available for capture of profits in the private sector. Comparisons among states' fiscal capacities either by examination of revenues by source or by use of the representative tax system (RTS) method lead to the conclusion that there is a notable gap between "have" and "have not" energy-producing states due to an uneven distribution of natural resources. This gives rise to a host of equity and efficiency issues regarding the consequences of taxation upon investment and production decisions as well as upon regional migration patterns and the provision of public goods. The Provincial Fiscal Equalization Program of the Canadian government demonstrates one approach to these issues whereby resource-poor provinces (as determined by deficiencies in fiscal capacity relative to the national average) receive transfers of federal revenue to compensate for differential revenue sources.

This book is a timely one and deals with a subject area that will continue to grow in importance as a theoretical and applied field. The format of the book includes comments upon each chapter by one or more scholars, and these are instructive critiques as well as sources of additional analysis. One deficiency of the book is its inattention to the costs of social and environmental externalities associated with the exploitation of natural resources as, for example, are discussed by John V. Krutilla and Anthony C. Fisher in *Economic and Fiscal Impacts of Coal Development* (Johns Hopkins University Press, 1977). Further, there is only a cursory treatment of the options for distribution of energy revenues received by states and provinces such as tax reductions, enhanced public services, or creation of trust funds for future and/or present investment purposes.

KATHLEEN O. JACKSON

University of Montana

Uncle Sam's Private, Profitseeking Corporations. By Lloyd Musolf. (Lexington, Mass.: D.C. Heath, 1982. Pp. vii + 126. $18.95.)

There is an important debate that is ongoing among students of public organizations. It concerns the ways in which public organizations are different than private organizations and how some public organizations are different than other public organizations. This thin volume has something to say on both counts.

The book examines a unique type of organization—the private, profitseeking corporation that has been created by an act of Congress rather than a stock issue. This type of organization is clearly close to the line where public becomes private, assuming this is a dichotomy. It may be well to remember Amitai Etzioni's admonition that dichotomies are the curse of the thinking person. Private, profit-seeking, and public seem more like a contradiction rather than a point on a public-private continuum.

The four organizations that Musolf examines—Comsat, Fannie Mae, Amtrak, and Conrail—come close to a census of this type of organization, as there are only six in existence in the U.S. All are quite different than the average bureau that is the standard fare of most studies of public organizations.

One of the most intriguing points made is that the source of funding is a critical variable in explaining behavior of Comsat and Fannie Mae versus Conrail and Amtrak. Both Conrail and Amtrak rely heavily on direct government subsidy. Each suffers from political interference, decisions based on political rather than an economic rationale, and an unending concern with maintaining the subsidy. It has been said of Amtrak that politics is the only reason it exists, and it would be far cheaper to close it and give everyone who called for reservations a bus ticket and $10.

Comsat and Fannie Mae, while in vastly different "businesses" (satellite communication and the secondary mortgage market), both display behavior usually associated with for-profit firms. If source of funding is the key ingredient in defining "publicness," then it supports the view expressed in Gary L. Wamsley and Mayer N. Zald's *The Political Economy of Public Organizations* (D.C. Heath, 1973) that organizations dependent on public funding exhibit characteristics associated with bureaus in the subsystem politics literature. Others have suggested that the key to the distinction is whether the good produced is collective or divisible in nature.

Musolf suggests another explanation that certainly accounts for some of the variance between the four organizations. Comsat was new, innovative, and aided by technological breakthroughs that brought down the projected costs of operation. Fannie Mae had a successful history before it geared up to handle the increased volume of federally guaranteed mortgages in the 1960s and 1970s. By contrast, Amtrak and Conrail became public institutions because of bankruptcy, decline, and neglect.

Interesting as the question of "publicness" is, it is not Musolf's intention to directly add to our knowledge on this score. His task is to trace the development of the government-created, private, profit-seeking corporation through an examination of the legislative histories surrounding each of the four corporations.

On this score the book could serve as an introduction to any one of the organizations, however, it cannot stand as either a detailed examination of any one or as a comparative study of one type of organization. The book is short on the kind of analysis that would make it a comparative case study. However, the book is like a good appetizer that satisfies one's initial hunger and whets the appetite for more substantial fare.

H. BRINTON MILWARD

University of Kentucky

The Social Constraints on Energy-Policy Implementation. Edited by Max Neiman and Barbara J. Burt. (Lexington, Mass.: D.C. Heath, 1983. Pp. vii + 213. $28.95.)

Virtually all books concerned with energy policy begin by noting the lack of a national comprehensive energy plan and end by recommending that the federal government adopt one of two energy strategies—either government-encouraged conservation or a free market, the choice depending on the author's ideological bent. Neiman and Burt are to be commended for their selection of articles, which illustrate well the scope, the complexity, and the major political and social obstacles to the development of rational energy policies at all levels of government. The articles range from an economic analysis of the federal government's oil and gas leasing program to a case study of the impact of transit station location upon energy use.

Of particular note are the articles by Michael D. Reagan and Michael F. Sheehan. Reagan's article traces many of the barriers to the formation of a national energy policy to the federal system and the attending problems of intergovernmental relations. According to Reagan, the lack of a national consensus on an appropriate energy strategy is exacerbated by the federal system and the necessary reliance of the national government on the states for policy implementation. Sheehan focuses upon the recent state and federal regulation of the electric utility industry and the resulting problems of policy implementation. His argument that government policy encouraged the electric power companies to embark upon a disastrous excessive capacity building during the 1970s is quite persuasive. Sheehan's proposed rate reforms concomitent with his suggestion that the industry be permitted diversification is equally convincing.

Several of the other selections report attempts by local governments to implement energy conserving legislation. Although the various authors lament the general ineffectiveness of these policies, they overlook one of the most significant reason for this failure. Despite the rising costs of energy in recent years, for most governments the proportion of their budgets devoted to energy remains relatively small. Thus the incentives for conservation for most units are lacking.

The serious researcher in energy policy implementation and assessment would do well to examine the selections of Neiman and Burt. They would also find the proposition inventory in the article by Gerry Riposa to be extremely helpful. In addition, those policymakers involved in policy formulation could benefit from the research reported here.

The rather terse introduction by the editors to each of the selections detracts from the book as a whole. It would have been most helpful to the lay reader, or the scholar just beginning to research this significant policy issue, had the editors provided a more thorough literature review in their introduction, followed by comments explaining how the included articles extend our knowledge of policy implementation. Except for this limitation, the book is a significant addition to the substantive issue of energy policy and the more general problem of policy implementation.

DAVID D. DABELKO

Ohio University

The Reagan Experiment. Edited by John L. Palmer and Isabel V. Sawhill. (Washington, D.C.: Urban Institute Press, 1982. Pp. vii + 530. $29.95, cloth; $12.95, paper.)

In *The Reagan Experiment* John Palmer, Isabel Sawhill, and their Urban Institute colleagues set forth the Reagan administration's domestic policy goals, outline the specific policies designed to achieve them, and examine the evidence to determine whether the policies have produced the desired results.

As Palmer and Sawhill state in the initial chapter, the administration's goals are clear: to generate economic recovery, to provide a stronger national defense, and "to transfer significant powers and responsibilities to state and local government, state and local governments and the authors' careful analysis of the policies aimed at reaching these goals begins with an examination of the tax and regulatory policies that are the essential elements of President Reagan's program for economic recovery. The focus then shifts to the changing relationships between the national government, state and local governments, and the nonprofit sector. The administration social policies in a number of areas such as health and education are examined. The concluding chapters are devoted to an assessment of the overall impact of the Reagan program on both regions of the country and on individuals.

This is a thorough and scholarly piece of work. Each of the authors studiously avoids partisan comments, although occasionally, as in the section on "Income Security" by James R. Storey, one has the distinct sensation of indignation on the part of an author as he/she describes the effects of policies like the "Safety Net."

In some areas, one finds the results of administration policies in keeping with the stated goals. For example, the authors find some real gains in both savings and productivity as a result of the Economic Recovery Tax Act of 1981, although the tax cuts under this act will clearly never be self-financing as true supply-siders had predicted them to be (chap. 4). Also, some small savings for business have probably resulted from a decrease in new government regulatory activity (chap. 5).

Some obvious failures of policy also appear. Although the administration desperately wanted to achieve a balanced budget, the tax and budget policies have interacted in such a way as to make the goal unreachable. The research indeed confirms the widely held opinion that it is impossible to enact nondefense spending cuts large enough to offset the revenues lost from the tax cut and required by the defense buildup (chap. 1).

In some places administration actions conflict with stated policy goals. A major effort, for example, has been made to provide "regulatory relief" for both public and private sectors through such measures as strict cost-benefit accounting, staff cuts, and suspension of regulations. Yet the number of rules and regulations applied to welfare programs has actually increased. And proposed deregulation of the trucking industry has been shelved as a result of industry pressure (chap. 5).

One result of Reagan policies is certainly clear. In the matter of tax cuts, employment training, social service, health, education, income security, and housing, the neediest recipients are those who will suffer most from program changes. Indeed, the much-advertised "safety net" for the "truly needy" does not include many on the edges of poverty such as employed welfare recipients and the long-term unemployed. As Storey states, the net has "proved to be a term that categorized people not by their degree of current financial need nor by their vulnerability to future economic insecurity. Rather, it appears to have been used primarily to delineate and protect the benefits of those for whom cutbacks would likely have aroused the strongest reaction in Congress" (p. 383).

Finally, it is obvious from the analyses found in *The Reagan Experiment* that the American political system has undergone profound social and economic changes since 1980, and that the legacy of the Reagan administration will shape governmental policy for many years to come.

MARGARET V. MOODY

Auburn University at Montgomery

Energy and the Western United States: Politics and Development. Edited by James L. Regens, Robert W. Rycroft, and Gregory A. Daneke. (New York: Praeger Publishers, 1982. Pp. viii + 195. $24.95.)

This book includes 10 chapters contributed by 14 authors or coauthors as well as a short eleventh summary chapter by the three editors. Morris Udall, in a very brief preface, sets the tone of the work. Most of the authors are political scientists, but other disciplines also are represented.

The major factors relating to energy and the American West are discussed: the potential for energy production (James Regens), land use (Timothy Hall), water and its scarcity (Steven Ballard), town planning and growth (David Hill and Daniel Schler, also Gregory Daneke), Indian tribes (David Schaller), federal versus state versus private control of land (Hanna Cortner and Helen Ingram), policies to mitigate energy development impacts (Allyn Brosz), and voting patterns of western state representatives and senators in Washington on energy issues (Henry Kenski and Michael Bowers).

Many chapters in *Energy and the Western United States* were originally papers given at various conferences; several were written under contract for government agencies. They are based primarily on research and data from the late 1970s and 1980 through 1981. Some limited rewriting and updating with 1982 information and trends was added when the papers were collected for the book.

Uncertainty and rapidly changing events create difficulties for this collection of readings. In many ways circumstances in 1983 and for the near future differ markedly from the environment of just a few years earlier. The emphasis of many of these chapters is that great energy developments are imminent, but these predictions of several years ago now seem postponed indefinitely. The changing times have particularly outdated the chapter "National Security: Synfuels and the MX System" by Robert Rycroft and James Monaghan. It focuses on the Carter administration MX proposals for a massive, wide-ranging missile system in Nevada and Utah. Now these proposals seem like ancient history and have been superseded by two quite different proposed configurations from the Reagan administration. Also, a huge synfuels program discussed in the chapter is today basically dead.

Several of the chapters could have been tied somewhat more explicitly to the theme of energy by relatively minor rewriting. For example, one of most interesting chapters is "The Case of the Sagebrush Rebellion" by Hanna Cortner and Helen Ingram, but the reader (at least this reviewer) is left to guess how the chapter relates directly to energy. A simple explanation probably could have integrated this chapter more successfully with energy development.

In their short summary the editors do note that "the conventional wisdom of the 1980s as an energy decade increasingly appears to have been a mirage" (p. 183). And they concede that the "concerns that prompted this volume may not present themselves in the precise context and configuration we initially envisioned" (p. 185), that is, huge, fast-paced energy developments causing enormous dislocations in the region.

They plead correctly, however, that the "long-range prospects for energy policy remain much the same" (p. 186). The Western states have been granted a reprieve: "the current hiatus provides a unique opportunity for cooperative planning" (p. 187). They suggest, too, that not a great deal of future preparation by either public or private sectors will be done until another future burst of energy development has inevasible momentum. The problems of handling the topic of energy development plainly reveal the frustration and confusion of attempting to analyze and plan for unpredictable and rapid political, economic, and technological changes.

DANIEL SLOAN

University of Colorado at Boulder

Public Administration and Law: Bench v. Bureau in the United States. By David H. Rosenbloom. (New York: Marcel Dekker, 1983. Pp. x + 236. $27.50.)

This is a good book and an important one. It is good because it meets its stated goals; important because its goals are important. These goals are stated in the Preface, where Rosenbloom tells us "the task of this book" is to meet a threefold need in the field of public administration. "What is needed is an approach that places the study of law squarely within the context of public administration, focuses on constitutional law and values, and also explains the radical expansion of the judicial role in public administration" (p. v). The author succeeds in meeting each of these needs.

He "places the study of law squarely within the context of public administration" by integrating law and administration rather than simply juxtaposing them. The first two chapters examine administrative history and the federal judiciary; but each of these topics is examined in terms of its relationship to the other. For example, the traditional counter-majoritarian problem associated with judicial review of acts of Congress is modified skillfully when applied to judicial review of administrative regulations. This integrating pattern is followed throughout the book. Each of the four chapters that treat topics of public law begins by establishing the inherent connection between the legal issue at hand and familiar administrative practices.

Rosenbloom's second task is to focus on "constitutional law and values." This is done by contrasting the bureaucratic values of the administrative state ("hierarchy, seniority, command, unity, and differentiated expertise") with the constitutional and democratic values of "equality, rotation-in-office, freedom, pluralism, and the presumption that all citizens are sufficiently competent (i.e., expert) to take part in elections and politics" (p. 19). This contrast is the major theme of the book. It is sustained convincingly throughout four thoughtful chapters that examine the administrative state in its relationship to its clients, its employees, its captives, and its antagonists in litigation. What unifies these chapters is a strong normative argument that administrators should accommodate their traditional values of economy and efficiency to the constitutional imperatives of diversity and personal liberty.

Rosenbloom's third task is to explain the "radical expansion of the judicial role in public administration." Here he presents a fascinating argument. It is the judiciary's traditional concern with individual rights that grounds its claim to join with Congress and the president in controlling the administrative state and thereby to preserve its coequal constitutional position. To do this, the courts have created new constitutional rights for clients, employees, and captives of the administrative state. These substantive rights have been supplemented with a new and considerably relaxed doctrine of official immunity that enables the recipients of these rights to make them effective in litigation. In a brilliant concluding section, Rosenbloom argues that the link between the courts' efforts in creating new rights and remedies explains "the fundamental coherence of the judicial response to the rise of the administrative state" (p. 213).

The only serious flaw in this book is that Rosenbloom's view of constitutional values is too narrow. He approaches the Constitution as though its purpose were simply to limit a government rather than create one and sustain it in being. This is understandable given Rosenbloom's strong interest in the courts, but it has the disadvantage of emphasizing the amendments to the Constitution at the expense of the Constitution itself. At times Rosenbloom tends to collapse constitutional values into judicial values and this, in turn, leads him to overstate the conflict between administration and the Constitution. The conflict is genuine; the author is absolutely convincing on this point. He goes too far, however, when he says the framers of the Constitution established a government that was "highly inefficient by design," and that a "governmental capability to act quickly and to implement forcefully was precisely what the founders hoped to avoid" (p. 77). Rosenbloom gives too little attention to the salience within the constitutional tradition of Publius' powerful argument that it is only a government that is vigorous, firm, efficient, and energetic that will be able to protect individual rights.

JOHN A. ROHR

Virginia Polytechnic Institute

Suing Government: Citizen Remedies for Official Wrongs. By Peter H. Schuck. (New Haven, Conn.: Yale University Press, 1983. Pp. xxi + 262. $25.00.)

"Lawbreaking is endemic to government" (p. xi). With these sobering introductory words Peter Schuck matter-of-factly sets the proper tone for his book about society's efforts to mitigate the harsh reality of official misconduct. Schuck argues that remedies for governmental lawlessness should promote four values: vigorous decision making, compensation, moral exemplification, and deterrence. In his view, the current piecemeal system of holding officials personally liable for government lawbreaking serves all these objectives quite poorly. Risk-averse bureaucrats dread the ordeal of personally responding to a civil suit. Vigorous decision making suffers as they become overly formal in the performance of their duties. Compensation and moral exemplification falter as plaintiffs succumb to a bewildering array of substantive and procedural barriers protecting official miscreants. Agency supervisors, moreover, have little incentive to deter future wrongdoing. The government itself, save for a few niggardly exceptions, is as immune from civil suit as the most despotic Tudor or Stuart.

At this point in the book, disturbing quantitative questions arise: How widespread is official lawbreaking? How much does official liability undermine each of the four criteria? Schuck attempts answers to questions of this sort, burying crude statistics in appendixes, in footnotes, and between parentheses. He effectively invites the reader to improve upon a shoddy data base, then continues a largely hypothetical analysis. For all its trenchant clarity and thoroughness, the case presented may be a "best" or "worst" case. The reader cannot tell.

Schuck follows his critiques with a prescription: Replace official liability with a new system of govermental liability. Bureaucrats, under this new regime, would no longer face the daunting prospect of a personal lawsuit. Plaintiffs, rid of the barriers currently protecting lawbreaking officials, would receive compensation from the deep pockets of government. Society would enjoy seeing justice done. Finally, agency heads, responding to budgetary pressures, would implement programs to deter expensive future misconduct. Schuck would accomplish this final trick by having the offending agency, rather than the general treasury, bear the burden of satisfying judgments for damages.

All this may sound a bit idyllic, but Schuck anticipates this charge. The final third of the book deals with practical implementation strategies. Once again, the argument displays a rigorous thoroughness that the fainthearted, overlooking the clarity of Schuck's prose, will mistake for tedium. But academic reformers seem fated to critique and prescribe with more cogency than they can muster later when they turn to implementation. Schuck ignores the fact that self-appointed fiscal watchdogs are already protesting the expense of the extremely modest forms of government liability currently in place. He accepts as inevitable practical accommodations with judicial and prosecutorial immunity that will offend civil libertarians. Finally, he urges "specific deterrence" (by means of rules, injunctions, and other coercive interventions) of the agency that comes to regard damages as a routine cost of doing business. This final discussion assumes a "judicial restraintist" posture of such Bickelian purity that the reader closes the book feeling defeated. Perhaps we cannot control official lawlessness after all; maybe the system is too fragmented and too much subject to unbridled discretion at isolated points.

This is a difficult and sobering book, one richly suggestive of specific policy recommendations and possible research projects. Anyone interested in the interplay of politics and law should read it.

Bert C. Buzan

California State University, Fullerton

Congressional Realignment, 1925-1978. By Barbara Sinclair. (Austin: University of Texas Press, 1982. Pp. ix + 201. $25.00.)

Barbara Sinclair has written a timely study of congressional realignment from 1925 to 1978. With President Reagan's efforts to remake the country's political agenda, her book provides a backdrop for discussing recent congressional actions.

Sinclair poses three questions in her research: What are the sources of agenda change? What determines congressional voting alignments? Under what conditions are the barriers to major policy changes overcome? Using roll call analysis, she explores the cohesiveness and durability of House voting patterns by party and region. Briefly, she finds that most instances of major agenda change come from strong and discrete environmental stimuli, that is, the Depression, war, electoral landslide, or social movements. Given the structure of the American political process, major external forces are necessary to overcome institutional fragmentation. However, legislators, the congressional process, and reelection constituencies can mediate the flow of events by influencing how environmental forces are interpreted and how party alignments are developed.

The strengths of this book are twofold. First, Sinclair deserves credit for putting her work in a theoretical framework. By using an agenda-setting approach, she has written a book that should interest scholars beyond the congressional roll call area. Second, unlike many congressional studies which are static in their orientation, she explicitly incorporates a change component in her analysis.

However, this book suffers from some of the limitations commonly associated with roll call analysis. Although the breadth of her coverage is impressive, Sinclair does not have any direct measure of constituency opinion (despite the fact that the notion of constituency plays an important role in her argument). In addition, the distinction between agenda, policy, and voting alignment changes sometimes becomes murky, since Sinclair uses roll calls to define all of them.

But these problems aside, Sinclair's analysis can be used to put recent congressional actions in an historical context. Although the book ends in 1978, before Reagan's efforts to redefine the national political agenda, her research offers some relevant ideas. By emphasizing the environmental stimulus of the 1980 elections and the important role that legislators played in interpreting Reagan's "mandate" for policy changes, one can put the policy actions of 1981 in a broader framework.

Because of its insight into past policy actions and its applicability to contemporary policy developments, I recommend Sinclair's book to scholars interested in congressional policymaking, agenda-setting, and roll call analysis. The study is well-written and clearly organized, and it deserves careful attention.

DARRELL M. WEST

Brown University

The Political Economy of Public Policy. Edited by Alan Stone and Edward J. Harpham. (Beverly Hills, Calif.: Sage Publications, 1982. Pp. 272. $25.00, cloth; $12.50, paper.)

This volume consists of an editors' introduction, 10 essays on themes related to political economy (generously defined in several instances), with subject and name indexes. The essays are uniformly well-edited, and several have considerable substantive merit. Part 1, "Ideology and the Political Economy" with chapters by Robert D. Holsworth and Raymond M. Seidelman, is the only really regrettable lapse in the generally informative anthology. Holsworth excoriates liberals for their incorrigible tendency at pragmatic compromise with the forces of reaction. Seidelman soundly thrashes the "corporatists" (a curious amalgam of institutions and personalities including, among others, *Business Week*, Lester Thurow, Felix Rohatyn, and Walter Mondale) for their unprogressive propensities. Since neither author takes alternative views seriously, their essays are more in the nature of journalistic scoldings than useful delineations of contending ideologies bearing on political economy.

More intreresting is Charles Noble's discussion of what he takes to be the failure of OSHA to fulfill its promise given the alleged dependence of the state on the capitalist system. Unfortunately, a data analysis of health and accidents trends in industry, either in the U.S. over time or between capitalist and socialist systems, is not presented, perhaps for reasons of space, making it impossible to assess whatever degree of failure may be involved. In his subtle essay, Alfred A. Marcus updates the position of the "new class" in American politics. He makes a strong, if not entirely convincing, case that the putative power of this privileged but adversarial "class," whose existence has been given popular form by a number of prominent writers, has been greatly exaggerated and scattered and defeated by far more coherent and organized business forces in recent years. Elizabeth Sanders seeks to explain the development of the New Deal's social welfare and regulatory institutions. Although insights and leads are offered, the subject is far too complicated for such cursory, restricted treatment. It is useful, however, to note the discussion as a point of view worth further examination.

Kenneth J. Meier criticizes cost-benefit analysis for familiar deficiencies and makes the plea that politics is a superior form of policymaking. One can agree somewhat without rejecting altogether, as Meier appears to do, such technical aids to analysis. Markets, as an alternative to both politics and cost-benefit analysis, are not mentioned.

The book's most sophisticated essay on both empirical and analytic planes is Richard P. Barke's study of railroad abandonment decisions by the ICC. Barke avoids entirely the fallacy of confusing consequence with sequence that intrudes in several of the other essays and is common these days in much radical or critical scholarship. Genuinely new information is generated and a plausible political and administrative account is given for the decisions taken. Gerald Epstein advances an innovative if preliminary theory of Federal Reserve Board behavior that contrasts its formal role as an "independent" stabilizing agent with its alleged control by the banking industry. Kenneth Woodside writes on the corporation tax in Canada and Britain as symbolic politics. Gary Freeman and Paul Adams's essay is an excellent source for those interested in the growth, difficulties, and justifications of the Social Security system.

Most chapters are rich with references to related literature, a great help for those who would like to explore particular themes at greater length.

L. L. WADE

University of California, Davis

The War-Making Powers of the President: Constitutional and International Law Aspects. By Ann Van Wynen Thomas and A. J. Thomas, Jr. (Dallas: SMU Press, 1982. Pp. v + 177. $15.00.)

In almost every administration, questions arise over who has the authority to commit U.S. forces to armed conflict (military action short of a declared war). In this relatively short work, the Thomases attempt to sort out the constitutional and international law aspects of the war powers.

In the first five chapters the authors present a good, but very sketchy (the first five chapters take only 33 pages), historical review of the war powers of the president as conceived by the Founders and as practiced through history. The brevity of these chapters makes them of only limited utility.

The book's substance is in chapter 6, "Constitutional and International Law Bases," in which the Thomases discuss the distinction between a formally declared war (war in the "legal sense"), a war in practice but undeclared (war in the "material sense"), and more limited hostilities. They conclude that "all theories permit some use of force by the President without congressional declaration. They differ on the degree of force which might permit the unilateral use or on the purpose of the force" (p. 49).

Given that there is no agreement on the amount of force a president may legally use in the absence of a declaration of war, what of the various components of the president's war powers? Here, there is some agreement. First, all parties agree that the congress has the power to "declare" war, and that once a war is declared, the president, as Commander-in-Chief, is responsible for conducting the war. Can Congress grant war-making power to the president without a declaration of war? Yes, "a formal congressional declaration of war is not necessary to express the consent of Congress to military action of a warlike nature" (p. 87).

What of "self-defense?" Has the president the power to repel sudden attacks? Again, the answer is yes (on practical if not Constitutional grounds). Can the president use the military to "suppress insurrection?" Yes, say the Thomases (p. 70). What of indirect threats to the nation's sovereignty? Here the problem becomes clouded.

Could, for example, the president use military force to protect the lives of U.S. citizens abroad? The Thomases say that in international law the answer is a clear yes, and in constitutional law the answer is a probable yes. What of the use of force to protect U.S. property abroad? Only, say the Thomases, if the "security of the state" were also endangered. Then, both international and constitutional law would permit the use of force by the president. Do American commitments based on mutual defense treaties with other countries allow the president to use military force? In international law, yes, but such a use of force might be unacceptable from a constitutional perspective. Treaties and executive agreements would seem to allow for the use of force in international law, but in constitutional law, it is not clear whether the president may use such force.

Does the president have independent war-making powers deriving from his role as Commander-in-Chief? The Thomases say that the president "may" (p. 71) have some powers here, but it is also unclear.

The final chapter, "Congressional Attempts to Curb Presidential Power," focuses on one of the most important parts of the war-making problem: conflict and/or cooperation between the president and Congress. Unfortunately, the authors devote almost all of their attention to the conflict in Southeast Asia, and, while they do an excellent job of describing president-curbing efforts by the Congress in this period, a more historical perspective or review is needed to get a fuller picture of this problem.

In chapter 7, the Thomases look at the impact of the "War Powers Resolution of 1973." Citing the fact that since its passage, presidents have complied with the Resolution while claiming to do so only as a courtesy, the authors contend that "future Presidents could presumably bypass the resolution by claiming they are acting under other constitutional grants than that to the Commander-in-Chief" (p. 133).

In looking at the constitutional and international law restraints on the president's war-making power, the authors beg certain questions. To what extent *are* presidents constrained by the Constitution or by international law? To what extent does the Constitution *really matter* when dealing with a resolute president determined to act?

Since there is no conclusion in the book, we are not given the benefit of a summation of just what power the president has in war making, but since the Thomases present no clear view on what the proper limits of presidential power might be, I think that we can assume that "the power of the President to commit forces abroad remains a dark continent of American jurisprudence" (p. 146).

MICHAEL A. GENOVESE

Loyola Marymount University

The Fall of the First British Empire: Origins of the War of American Independence. By Robert W. Tucker and David C. Hendrickson. (Baltimore: Johns Hopkins University Press, 1982. Pp. viii + 450. $24.00.)

The Fall of the First British Empire covers the period from the peace settlement of 1763 following the Seven Years' War to the outbreak of hostilities at Lexington and Concord in the spring of 1775. The central concern of the authors is to explain how the First British Empire, at its zenith in 1763 with victory over the French in North America, broke up just over a decade later with the American colonies in armed revolt. In the course of this investigation Tucker and Hendrickson challenge the analysis and interpretation of nineteenth- and twentieth-century historical scholarship.

The major criticism made by the authors of this historiography is that it accepted the colonists' position of the conflict. Both colonists and later historians accepted the position that the crisis between the colonists and Britain was due to British imperial policy, post-1763, which trampled upon the rights and autonomy the colonies had enjoyed previously. It was thus the British attempt to embark upon a policy of imperial reform which, by violating long-established colonial-imperial relationships, destroyed the status quo and precipitated colonial reaction and resistance.

The error in this interpretation, according to Tucker and Hendrickson, is the failure of historians to recognize that "there were two very different understandings of the status quo" (p. 74). The colonial view of the status quo resisted "almost any imperial reform, however limited and discrete its character," while in the British "understanding of the status quo, there were no formally acknowleded limitations to the authority it [Britain] claimed to exercise as of right over the colonies" (p. 73). The fault of "a long-established tradition of historical scholarship" was to identify "the colonial version of the status quo with *the* status quo. . ." (p. 74).

As the status quo issue is the focus of the first one-half of the book, the second half is devoted to the British reaction to colonial protest of British imperial policy. This reaction is characterized as "The Diplomacy of Appeasement," the title of Part 3 of the book. The authors contend that each British ministry from 1765 to 1773 (Rockingham, Chatham, Graften, North) is guilty of the policy of appeasement. Throughout this period, successive British ministries are described as exhibiting "passivity," "conciliation," "inconstancy," "drift," "steady retreat," "giving way," and having "lack of will" in the face of colonial resistance.

What accounts for this policy of appeasement by successive British governments spanning a decade of colonial-imperial relationships? The answer, according to the authors, is the existence in Britain of a "twofold consensus—to avoid the armed coercion of America and to maintain the supremacy of Parliament. . ." (p. 255). It was the obtuseness of British ministries that prevented them from recognizing that this "twofold consensus" comprised irreconcilable goals. It was not until 1775 that the British realized the impossibility of maintaining the supremacy of Parliament without forceful coercion of the colonies. Tucker and Hendrickson are unsparing in their indictment of the British policy of appeasement that prevailed for more than a decade after 1763.

Implicit in this indictment is the impression that the authors convey that the fall of the First British Empire could have been prevented if, at some point during the decade, Britain had summoned the resolve to bring the colonists to heel by armed force, if necessary. But even if this resolve could have been mustered, did Britain have the muscle to implement it?

What is frustratingly missing in Tucker and Hendrickson's provocative analysis of the fall of the Empire is any attempt to suggest how, even if Britain had the "will" to preserve it, the British could have found a "way" (resources) to do it. The authors say that appeasement failed to preserve the Empire. Could the application of force before 1774 have saved it? If so, Tucker and Hendrickson do not say how.

JOHN S. VANDEROEF

The Florida State University

The Vice President as Policy Maker: Rockefeller in the Ford White House. By Michael Turner. (Westport, Conn.: Greenwood Press, 1982. Pp. xvii + 252. $29.95.)

Nelson Rockefeller's attempt to bring the office of vice-president into the mainstream of policymaking is the subject of this short book. Turner concludes that Rockefeller was admirably successful in involving himself in the policymaking process. The reader, however, is impressed by how remarkably little was achieved by such involvement.

Using both extensive documentary resources and interviews with the Ford-Rockefeller staffs, Turner examines Rockefeller's involvement in four policy areas: the creation of a White House science advisory unit (approved by both the president and Congress), the 1976 domestic program policy options list (totally ignored), the creation of an energy development finance corporation (killed by Congress, approved in the Carter administration), and aid to New York City during its fiscal crisis (publicly repudiated by President Ford but eventually implemented). Rockefeller was actively involved in the development of these policies. His positions were, however, so compromised, resisted, or fought that at one point he threatened to quit unless he started getting cooperation.

Turner does an excellent job of delineating the factors that may impede any vice-president's policy involvement efforts. It is possible to group these factors (although Turner doesn't) into three groups: peculiar, situational, and personal. The first involves Rockefeller's confirmation, as required by the Twenty-Fifth Amendment, which took four months. In the interim, Ford's staff had developed their own procedures and were unwilling to adapt when Rockefeller assumed office. Turner believes (p. 157) that the domestic policy options list and the energy corporation may have been more successful if Rockefeller had had more time to push for them.

The second, situational, group of factors refers to the nature of the president, the "normal" selection process, and the office of the vice-presidency itself. Ford is repeatedly characterized as imprecise, ambivalent, and indecisive. As a result, Rockefeller's role was never clearly articulated or communicated to the staff with whom Rockefeller was to work. And, as the vice-president's relations with the staff deteriorated, Ford would not intervene until Rockefeller personally appealed for help. In this case the president's nature undercut the vice-president's role and authority. Furthermore, says Turner, by the time the vice-president is selected at the convention, the future president has been working with his staff for approximately a year. Finally, the office of the vice-president is not provided with sufficient resources for independent policymaking activity. Rather, the vice-president must rely upon already installed, and probably hostile, mechanisms.

Finally, in Rockefeller's case, personal factors contributed to his difficulties. He simply could not act like a subordinate, a staff member. Having served as a four-term governor, he was used to acting as a chief executive. Such was not appreciated by Ford's staff.

Turner makes essentially two concluding recommendations. First, the president and vice-president must agree on and publish the vice-president's role. Second, they should hold weekly meetings, the agenda of which would be controlled by the vice-president.

RAYMOND L. CHAMBERS

Bainbridge Junior College

The House and Foreign Policy. By Charles W. Whalen, Jr. (Chapel Hill: University of North Carolina Press, 1982. Pp. x + 207. $18.95, cloth; $9.95, paper.)

Charles Whalen examines the reassertion of the congressional role in the conduct of American foreign policy. He concentrates on the House of Representatives' procedural reforms implemented during the seventies and their impact on the substance of policy.

Whalen finds that there were three major, mutually reinforcing institutional changes: decentralization of power, greater openness in procedures, and a strengthened capacity to deal with the executive branch (for example, the War Powers Act). Although Whalen finds most of these reforms well-intentioned, he believes that they have "led to actions that ill-served our nation's interests abroad" (p. 48).

Whalen argues that the House has become too decentralized, too open, and, consequently, too responsive to the public. "The capacity of House officials to lead has been constricted, the executive branch's ability to 'persuade' has diminished, and members have become more vulnerable to the often uninformed judgement of the public they serve" (p. 77). In other words, the electoral connection has become the dominant day-to-day concern of most congressmen. Unfortunately, Whalen addresses, but does not attempt to resolve, one of the perennial problems of an open society: the reconciliation of representation with a concern for good public policy.

Whalen describes major instances in which the House has become actively involved in the making of foreign policy. He concludes that while successfully "diminishing potential misadventures by the executive branch, the House's new operational methods have often bred legislative excesses and contributed to House actions that have repudiated long-standing global obligations, threatened the continued existence of several international organizations, and offended many friendly nations in both hemispheres" (p. 172).

Whalen, himself a former congressman, rejects as impractical most of the "reforms of the reforms" that have been suggested. He offers three modest proposals which he acknowledges, at best, would only *"diminish the opportunity for irresponsibility."*

Overall, the book provides a fine overview of the congressional role in foreign policy, although it does not break new ground. It seems to me that a focus on the role of procedural matters, although important, misses the crucial factor responsible for the continued activism of Congress. Vietnam destroyed whatever foreign policy consensus that had previously existed in American society. It is the breakdown of consensus that has catapulted congressional members into becoming involved in the foreign policy process. Reforming the institution once again will not alter this basic fact, although it may diminish some of the excesses. As long as a foreign policy belief system fails to become dominant and consensus is not regained throughout American society, congressional attention and involvement will remain high —as the Reagan administration has experienced in its Central American policy.

JEREL A. ROSATI

University of South Carolina

Managing Crisis Cities: The New Black Leadership and the Politics of Resource Allocation. By Bette Woody. (Westport, Conn.: Greenwood Press, 1982. Pp. xviii + 228. $27.50.)

Much of the urban literature over the past decade can be categorized into one of two classes: that which deals with urban phenomena generally and contains broad assessments of evolutionary/devolutionary trends, and that which focuses on the problems of a specific city or group of cities with homogeneous characteristics. The collection has been disappointing. The generalized approach invariably explains little in attempting to explain everything, while the narrowly focused studies are riddled with caveats cautioning against inducing broad findings from the restricted research foci.

Managing Crisis Cities struggles with this levels of analysis problem and enjoys a fair measure of success in overcoming it. Specifically, Woody surveys various urban policies and reforms during the 1970s and early 80s in an effort to explain their failure in resolving the urban fiscal crisis and its attendant problems. Particular attention is devoted to those cities that have experienced a change in leadership from ethnic-dominated political machinery to black coalitions. Bette Woody argues that the developmental trends in such cities are illustrative, because much of the success in urban governance and leadership in the future will be dependent upon "mediating black demands" (p. 20).

From this initial thesis, Woody turns to an in-depth analysis of five cities during the administration of black mayors: Los Angeles, New Orleans, Oakland, Detroit, and Newark. Reform strategies in each of these cities are examined, along with some of the political, economic, and structural constraints that have hindered reformism. The issues of service cost and cost distribution provide a recurring theme as well as a basis for comparison.

Finally, Woody considers the implication of black-run cities from the perspective of developing a successful national urban policy. Her observation that U.S. cities generally have suffered from a lack of "institutional status and legitimacy in the national system" (p. 207) is hardly novel. Yet her reasoning, which incorporates a criticism of the self-reliance ethic historically plaguing cities, is persuasive.

On the whole, *Managing Crisis Cities* is carefully researched and well written. The book presents a number of empirical and analytical findings derived from primary source documents as well as secondary literature. The discussion of contemporary urban reform initiatives is both comprehensive and insightful. Moreover, Woody's style is fluid enough to permit a shift in orientation from the past to the present to the future without disrupting the continuity in the theme.

Unfortunately, there are two shortcomings that serve to undermine the effectiveness of the book. First, a reader is compelled to accept Woody's assumptions regarding the benchmark nature of black-run cities in order for the subsequent analysis and conclusions to be broadly applicable to the urban fiscal crisis. It is evident that she is cognizant of this concern. In chapter 2 in particular she discusses at length her selection of black-run cities and their importance in explaining allocative decisions. Second, the book's scope of inquiry is perhaps overly ambitious. In attempting to explain the causes of fiscal crisis and proffer remedies, Woody discusses leadership styles, budgetary strategies, service delivery mechanisms, administrative control techniques, and a variety of other topics. Granted, each of these is a critical factor in both causing and controlling fiscal stress. However, the book at points stretches its purview so broad that the depth of analysis necessarily suffers. Still, this approach is vastly superior to those which have offered simplistic and trite explanations for the urban fiscal crisis.

Despite these shortcomings, *Managing Crisis Cities* warrants attention. Both urban and financial specialists will appreciate its focus and its arguments.

LUTHER F. CARTER

College of Charleston

The Limits of Law Enforcement. By Hans Zeisel. (Chicago: University of Chicago Press, 1982. Pp. xvi + 245. $20.00.)

Conservative ideology dominates contemporary criminal justice policy. It seeks to repress crime through the efficient apprehension and conviction of criminals and their swift, certain, and severe punishment. *The Limits of Law Enforcement* by Hans Zeisel, professor emeritus at the University of Chicago Law School, provides strong evidence that this approach to crime is flawed for two reasons. First, little can be done to increase arrest and conviction rates. Second, the incapacitation and deterrent effects of more severe punishments are at best limited. "The truth is," writes Zeisel, "that law enforcement, important and essential as it is, cannot by itself significantly reduce crime" (p. xv).

The Limits of Law Enforcement has two objectives: the presentation of data regarding the disposition of felony arrests made by the New York

City Policy Department, and the identification of variables affecting the disposition of these felony arrests. The disposition data is derived from two probability samples of felony arrests disposed of without jury or bench trials. Zeisel follows these cases from arrest to disposition. He presents the data in lucid prose and easily understood graphs. His interpretation of the data is incisive. One general conclusion emerges from Zeisel's analysis of arrest rates, dismissal rates, and plea bargaining: It is erroneous to blame the police and the courts for the high rate of crime. Three general findings presented in *The Limits of Law Enforcement* support this conclusion. First, police operations have a limited impact on crime primarily because many crimes are not reported to the police. And when arrests occur, many are dismissed at the preliminary hearing because the complaining witnesses either withdraw or lack credibility. Second, the judicial system is efficient in securing convictions. Of those cases not dismissed, 96% end in guilty pleas. The key inducement to plead guilty is a reduction in sentence. For inmates sentenced to prison, the average reduction between the sentence offered and refused during plea negotiations and the sentence finally imposed is 42%. Third, the reduction in sentence has only a small impact on crime rates. The incapacitation effect of doubling the current length of prison sentences is modest; selective incapacitation is hobbled by our inability to identify habitual criminals early in their careers in crime. Similarly, the deterrent effect of longer sentences is marginal.

Zeisel correctly grasps the policy implications of his findings: "Crime control, to become effective, must look beyond [law] enforcement" (p. 84). Recent research supports his conclusion. The formal instruments of criminal justice are relatively powerless to reduce the overall level of crime. Solutions outside the criminal justice system must be sought. Zeisel advocates renewed efforts to eradicate the social roots of crime. Regrettably, *The Limits of Law Enforcement* adds little to our knowledge of the origins of criminal behavior. His identifying the ghetto as the "core of the problem" (p. 75) is based more upon its correlation with high crime rates than with existing knowledge of its causal relationship with crime. Nevertheless, *The Limits of Law Enforcement* indicates that much has been learned about crime prevention since the outset of the "war on crime": Faster police cars and tougher punishments have limited impact on crime rates. Central to a successful strategy of crime control is a strengthening of the informal mechanisms of social control. In an era marked by overcrowded prisons, longer prison terms, and a lessening of constitutional restraints on the police, *The Limits of Law Enforcement* is worthy of study and reflection.

JAMES E. ROBERTSON

Mankato State University

State-Local Relations: A Partnership Approach. By Joseph F. Zimmerman. (New York: Praeger Publishers, 1983. Pp. viii + 240. $28.95.)

Students of American intergovernmental relations have long neglected the area of state-local relationships. With a few notable exceptions, there has been little systematic research attention to the subject. Textbooks and courses give state-local relations cursory treatment. Perhaps the scholarly neglect stems from the difficulty of the research task, with remarkable diversity found among and within the 50 states. With this admirable volume, Zimmerman steps bravely into this void. Some of the material stems from studies Zimmerman conducted for the U.S. Advisory Commission on Intergovernmental Relations (ACIR).

Issues of state-local relations raise classic questions of centralization versus decentralization in American democracy. Typically, we are ambivalent on these issues. On the one hand, we want the state government to have sufficient power to accomplish necessary tasks. On the other, we cling to revered notions of grass-roots democracy. Zimmerman begins with a delineation of formal power distribution and a discussion of state-local tensions and problems.

Chapter 2 sets out the dimensions of the legal relationship between states and localities. Zimmerman contrasts two broad approaches in granting discretionary authority to local governments: the *Imperium in Imperio* (or state within a state) approach, which partially insulates local government from state interference, and the devolution of powers approach, which delegates many powers to general purpose local governments. Zimmerman also constructs interesting indexes of local discretionary authority.

The third chapter outlines state-local fiscal relations, with particular attention to state mandates and state grants-in-aid. The chapter includes discussions of constitutional and statutory fiscal provisions and the property tax. In brief, Zimmerman's research finds that state mandates are the major irritant in state-local relations, and that local governments have organized in reaction to increased mandated costs.

Chapter 4 is a fascinating presentation of state responses to local problems. Zimmerman notes three major state roles in response to urban problems: inhibitor, facilitator, and initiator. Perhaps we are most familiar with the states' inhibiting role in regard to area-wide government and fiscal restraints. States have made the solution of metropolitan problems easy in numerous ways, including the creation of regional planning commissions and the establishment of municipal bond banks and investment and insurance pools. States have also initiated many actions to solve local government problems, including the establishment of public authorities and incorporation and boundary changes.

In the fifth chapter, Zimmerman discusses increased federal influence in the state-local relations. The expansion of national powers and the impact of federal preemption in recent decades are reviewed. Naturally, federal financial assistance receives substantial attention. Zimmerman proposes a national tax credit program as a substitute for conditional grants-in-aid.

Zimmerman concludes with a model of state-local relations that is reformist and visionary. It includes proposals that would maximize local discretionary authority, such as devolution of all powers capable of devolution, state reimbursement of mandated costs, removal of debt and tax limits on local governments, and the creation of state boundary commissions.

One outstanding feature of the book is the extensive bibliography. Zimmerman provides a 44-page bibliography with more than 600 references, including books and monographs, government reports and documents, articles, and unpublished material. The appendixes set out Zimmerman's indexes of local discretionary authority.

Overall, Zimmerman's book is an excellent description of existing state-local relationships. Some may not like the prescriptive sections, while others may yearn for more extensive empirical work. The book would be highly suitable for classroom use in courses in intergovernmental relations and state-local government if the publisher issues a classroom edition. For the foreseeable future, this book undoubtedly will serve as a standard reference.

ROBERT K. WHELAN

University of New Orleans

Comparative and Other Area Studies

Local Government Finance in the Third World: A Case Study of the Philippines. Edited by Roy Bahl and Barbara D. Miller. (New York: Praeger Publishers, 1983. Pp. xvi + 269. $22.95.)

American theories of political and economic development usually affirm the value of decentralization so that rural areas are brought into national life and dangerously destabilizing socioeconomic gaps are mitigated. For their own reasons, political leaders of third-world nations also give rhetorical support to a devolution of money and power to the poorest of their people, usually those who live in small towns and remote areas. Filipino politicians from the days of the Commonwealth to President Marcos's "New Republic" have proposed and proclaimed numerous legal and fiscal measures to invigorate local governments. This carefully written and detailed book, edited by Bahl and Miller, reports on the present financial status of Philippine provinces, cities, and towns and proffers a number of reasonable suggestions for ways in which the system could be tinkered with and improved upon. In doing so, however, it ignores most of the real reasons for the financial and political impotence of local governments in the Philippines.

Within the scope of the problem that Roy Bahl, the principal author, has defined, his study is scholarly and objective. It analyzes local government structure, the real property tax, the business license tax, intergovernmental fiscal relations, local government private enterprise, and local government credit financing. The final chapter makes specific recommendations, most of which seem sound. Filipino leaders genuinely interested in strengthening local governments would be well-advised to follow Bahl's advice. The problem is, of course, that national Filipino leaders don't want decentralization, and neither do leaders in other third-world countries. Unfortunately, such "political" questions are not addressed in the book under review.

Bahl and his coauthors recognize that, despite the national government's oft-repeated commitment to strengthening local governments, the trend is in the opposite direction. Perhaps because this study was sponsored by the United States Agency for International Development, it refrains from stating the obvious: Philippine President Ferdinand Marcos has radically centralized political and economic power in his country. It is understandable, the island nation is geographically and culturally fragmented. Since multiethnicity is the rule rather than the exception in developing countries, we should not expect many modernizers to grant substantive powers to local governments.

Nonetheless, to maintain their base of support national leaders will undertake programs that give the appearance of conferring financial responsibility and autonomy on various levels of regional and local government. This is precisely what has happened in the Philippines, except that instead of utilizing existing governmental agencies, President and Mrs. Marcos have embarked on a new program, the *Kilusang Kabuhayan at Kaunlaran,* which will put up to $300 million in the hands of local government officials this year. But rather than infusing renewed vigor in local governments, this gigantic pork barrel project is aimed at cementing the pyramid of personalized power over which Marcos and his wife preside.

Bahl has prescribed effective medicine, but it is for symptomatic relief only.

ROSS MARLAY

Arkansas State University

Liberal Party Politics. Edited by Vernon Bogdanor. (New York: Oxford University Press, 1983. Pp. xv + 302. $37.50.)

The Politics of the Labour Party. Edited by Dennis Kavanagh. (Winchester, Mass.: Allen & Unwin, 1982. Pp. xi + 228. $27.50, cloth; $9.95, paper.)

The difficulty of offering a meaningful analysis of British party politics in this present era of change is rather neatly illustrated by the photos chosen for the cover of *The Politics of the Labour Party:* Michael Foot is accorded pride of place, flanked on the right by Denis Healey and on the left by Tony Benn. In the last year, however, Foot has been replaced as party leader by Neil Kinnock, Healey as deputy leader by Roy Hattersley, while Benn, although still the most prominent leader of the Labour left, has lost his seat in Westminster. Yet despite the present British political flux, the editors of both of these collections have managed to bring together a number of valuable essays which, although timely, seem not at all constrained by the passage of recent events. *Liberal Party Politics* is essentially a stock-taking exercise on the present state of the Liberal party on the eve of what may yet prove to be its major breakthrough to majority party status, while *The Politics of the Labour Party* is consciously less concerned with Labour's contemporary problems and is oriented more towards themes of a longer-term significance, many of which relate to Labour's steady and seemingly inexorable electoral decline.

Of the two collections, that edited by Kavanagh is clearly the more rigorous, particularly thanks to the excellent chapters by Ivor Crewe ("The Labour Party and the Electorate") and L. J. Sharpe ("Labour and the Geography of Inequality"), but also because of Kavanagh's own chapters on "elite recruitment" and "representation" and Colin Crouch's assessment of relations between the party and the unions. Other contributors include Philip Williams on changing styles of Labour leadership, Hugh Berrington on the increasing left-wing orientation of the party in Parliament, Paul Whiteley on the decline of local party membership, and the late Robert McKenzie on intraparty democracy.

The contributions to *Liberal Party Politics* are more uneven, the most valuable being those by Philip Norton on the party in Parliament and Andrew Gamble on "Liberals and the Economy," while other useful chapters come from Michael Brock and Peter Clarke on Liberal historical traditions, William Wallace on the party's postwar history, and John Curtice on sources of Liberal electoral support. The remaining chapters include Dennis Kavanagh on party organization, Michael Steed on electoral strategy, Alan Butt Philip on the Liberals and Europe, Bryan Keith-Lucas and Stuart Mole on local government and community politics, and Bogdanor himself on the Liberals and constitutional reform.

Both collections offer useful insights into the dilemmas of contemporary British party politics, and insofar as both identify similar reasons for the decline of Labour in the one case and the potential for a Liberal breakthrough in the other, they complement one another quite effectively. Nor is it surprising that common ground should be found, in that the crucial watershed in both cases seems to be the breakaway from Labour of the Social Democratic party and its alliance with the Liberals in an attempt to jointly win the center stage in British politics. But if the formation of the SDP was or is to prove the crucial watershed for British politics, it is the gradual erosion of a Labour support that is the most evident long-term trend. Both of these books were completed before the June, 1983 election, but already Labour's decline was quite evident: from 48% of the votes and 41% of the electorate in 1951 to just 37% of the votes and 28% of the electorate in 1979. (The respective figures for 1983 are 28% and 20%.)

The most telling analysis of Labour's decline is that by Crewe in *The Politics of the Labour Party* in which he identifies a dual process of demographic contraction *and* declining levels of Labour identification within the party's "natural" working-class constituency such that, in the 1980s, the party "faces a working-class electorate with only a minimal sense of class interest" (p. 23). If, at the same time, we accept Crouch's argument that "there is no social cleavage within the society to support a major degree of party competition other than that which is represented by the conflict between organised labour and capital" (p. 173), then the imputed decline of class politics is likely to create an essentially "destructured" electorate which, in turn, is seen to favor the Liberal party and the SPD.

Curtice points out in *Liberal Party Politics* that Liberal support is not now organized on class lines, while attempts to explain Liberal voting in terms of other social cleavages have proved either "indeterminate" or "fruitless": "Liberal support has a socially 'rootless' character" (p. 102). In such circumstances, the potential freeing of British politics from its traditional social bases could assist the growth of the Liberal party. In a similar vein, both Bogdanor and Wallace refer to Jo Grimond's original strategy for the Liberals in the early 1960s, which emphasized the need for a realignment of British politics into an opposition of the progressive or radical elements in all parties against conservatism, with possibly a small left-leaning Labour party on the fringes of the system. The emergence of the SDP and the growth in electoral support for the Liberal-SDP Alliance would now seem to have revived the chances for such a realignment. But Bogdanor sounds a warning note when he suggests (pp. 8-9) that the weakening of the ties that bind class to party may not lead to a realignment of British politics as such, but rather to a process of dealignment which may be of long-term benefit to none of the parties.

In the last analysis, none of the contributors to either collection is really willing to forecast future trends in British politics; all are more or less content to account for the present state of play and, indeed, both collections are very useful in this sense. Referring in *The Politics of the Labour Party* to the "glacially slow rate at which a major party in Britain is finally abandoned by the electorate," Crewe cautions that "prophecies of doom are clearly premature" (p. 17). Notwithstanding the June, 1983 election results, even the most ardent Liberal-SDP supporter must surely agree that all other prophecies in British politics are also clearly premature.

PETER MAIR

European University Institute, Florence

Khrushchev and Brezhnev as Leaders: Building Authority in Soviet Politics. By George W. Breslauer. (Winchester, Mass.: Allen & Unwin, 1982. Pp. xiii + 318. $28.50, cloth; $12.95, paper.)

Whereas most observers of Soviet elite politics focus primarily on power struggles and patronage, George Breslauer in this book takes a more sophisticated approach, looking instead at the dimension of authority building. He argues that the relaxation of terror following Stalin's death reduced the support that Soviet leaders could expect to gain from patronage and has led them to concentrate more on increasing and maintaining their political authority. Soviet leaders therefore have become more like their Western counterparts, having to prove their worth as effective politicians and problem solvers. Breslauer seeks to probe the different methods Khrushchev and Brezhnev used to build up their authority as leaders and to show how these approaches evolved over time.

Breslauer identifies the authority-building strategy of the post-Stalin leadership by carrying out an informal content analysis of their speeches. Each leader is examined independently to determine his position on the "grand issues" of Soviet domestic policy: investment priorities, incentive policy, administrative reform, and political participation. Breslauer traces how the policy priorities and leadership styles of Khrushchev and Brezhnev evolved as they struggled for succession, rose to ascendancy, and finally found their policy programs frustrated. In addition, he attempts to ascertain how much power each leader actually had over policy. The book concludes with an examination of the pattern of leadership since Stalin, which compares the two leaders and discusses the concept of leadership cycles.

This book fills a void in the literature by taking a long view of both Soviet administrations. It illustrates that each leader benefited from a broad elite consensus in favor of breaking with Stalinism, expanding political participation, and improving the system of public administration. Khrushchev and Brezhnev both emerge from this analysis as "party men" who defended the primacy of the Party and promoted programs designed around party activism. Each combined the goals of the new consensus with traditional Soviet values in formulating his program. In spite of these similarities, both men adopted distinctly different leadership strategies. Khrushchev is generally seen as a dedicated decentralizer who continually confronted major institutional interests in Soviet society, while Brezhnev is regarded as a conservative consensus-builder. While these general characterizations are widely held by Kremlinologists, Breslauer argues that they obscure or neglect important aspects of each leader's policies. He also challenges some of the more specific images of Khrushchev's and Brezhnev's leadership. This study contends that far from being an embattled consumer advocate, Khrushchev was inconsistent in his support of light industry. Looking at Brezhnev, the book shows that in addition to being a broker between major interests, he was also an important policy initiator. Breslauer's authority-building approach demonstrates that many of our images of these leaders need substantial revision or qualification.

What is missing from this study is a thorough analysis of foreign policy and an assessment of how it related to each leader's domestic policy program and authority-building strategy. Without examining the nexus between domestic and foreign policy it is impossible to obtain a complete picture of how both men acted as leaders. While Breslauer is sensitive to this issue, the book does not go far enough in addressing it.

This is an excellent study that demands the attention of students of Soviet politics. Breslauer's approach leads to some notable insights into Soviet leadership strategies. He presents a cogent and fresh analysis of Khrushchev and one of the more thoroughgoing studies of Brezhnev to date. Soviet scholars will find much of the nitty-gritty detail in this highly documented study to be both interesting and useful. The book makes a strong case that many changes in policy advocacy by Khrushchev and Brezhnev can only be understood when looked at as products of an authority-building strategy.

CAREY CAVANAUGH

Youngstown State University

Political Leadership in Africa. By John Cartwright. (New York: St. Martin's Press, 1983. Pp. 299. $27.50, paper.)

The substance of political leadership in Africa can be categorized in a number of ways. Cartwright offers two such schema, persuasion versus coercion and accommodation versus transformation. He uses seven case studies to illustrate the distinctions between these approaches.

The persuasive approach to political leadership is, according to Cartwright, more effective in the long term because it affects changes in people's attitudes, whereas coercive tactics create a passive restraint. It was the fate of the newly independent African states of the 1960s, however, that the temptations toward the use of coercion were great, and the weaknesses of governmental and economic institutions further contributed to such inclinations. In fact, Cartwright asserts, the sole restraint to the use of force is the leader's own, internalized faith in his leadership ability.

The distinction between the accommodationist and transformationist approaches relates to the means employed to seek long-term political and economic development. The accommodationist seeks to maintain order while domestic or foreign entrepreneurs seek profits which, in turn, provide some benefits to the polity and the indigenous population. The transformationist espouses a vision of a better future and attempts to inspire changes in the attitudes of his followers in order to pursue that vision of the future. In the former approach, accommodation has tended to leave the impression of a sellout to foreign interests, hardly a popular impression to give in postindependence Africa. The latter approach, transformation, requires not only the changing of peoples' attitudes but also the sacrifice of short-term comforts in favor of long-term potential benefits.

The central question for this research is whether any of the leaders studied could have been more effective had they pursued strategies other than those they selected. This may suggest a greater freedom to choose than was the case, however. It could be argued that to have selected alternate approaches would have required these leaders to pursue strategies not entirely consistent with the leaders' central needs and philosophies. Could Nyerere have abandoned his pursuit of a sustained charismatic image, or could Toure seek to reach an accommodation with the French?

Cartwright argues that the very newness of the African states and the lack of formal institutions gave postindependence leaders a wide latitude to select from among these alternatives. If, then, one assumes that such conscious decisions could have been made at the outset, the question becomes whether other approaches would have been more or less effective. Cartwright notes that each of these approaches has limitations, and each can lead to the decline of both the leader and the led, although he recognizes the need for a leader to obtain popular support for long-term change and that to do so requires persuasive leadership.

Persuasion or coercion notwithstanding, the point must be noted, perhaps with greater emphasis than provided in this work, that the independence struggle was relatively easy compared to the subsequent pursuit of political and economic development. There was a common goal and a common enemy. With independence in hand, the leaders lost their rallying cry. In short, to retain power in Africa in the period of 10 to 15 years after independence was achieved was either to produce results in terms of improved standards of living, or to convince the people to be satisfied with a vision of a better future. As Cartwright points out, some leaders developed comprehensive strategies, although most simply "drifted with events, responding to crises on an *ad hoc* basis" (p. 53). It is clear, then, that the method used to accomplish these ends is far less crucial than the ends themselves.

GERALD L. GORDON

Burke, Virginia

The Administrative State in Canada: Essays for J. E. Hodgetts. Edited by O. P. Dwivedi. (Toronto: University of Toronto Press, 1982. Pp. xi + 258. $30.00, cloth; $10.95, paper.)

J. E. Hodgetts has published eight books and more than 50 major papers on the public service in Canada. For most of his distinguished career he has centered his attention on the Ottawa bureaucracy, both analyzing its contemporary effectiveness and practice and describing its history. He has written major studies of the Civil Service Commission and has interpreted the findings of several royal commissions to his less attentive or persevering colleagues. Unlike in the U.S., in Canada there is relatively little intercourse between the government and the universities. Generally, when professors leave, it is for good. J. E. Hodgetts is unusual inasmuch as he has, in John Meisel's words, complemented his academic work and university preoccupations with sorties into the government orbit. More recently, Hodgetts has turned his attention to intergovernmental affairs and provincial bureaucracies.

The essays collected to honor this man reflect the scope of his interest and the focus of his enduring concerns. The first five deal with the question of administrative power. Kenneth Kernaghan and T. H. McLeod provide a sympathetic account of 1930 to 1960, the golden age of the Ottawa mandarins. 1960 was also a significant year in the history of the Quebec civil service. Roche Bolduc and James Iain Gow show that after that date the state began actively to influence the outside world instead of simply responding to external business, party, and ecclesiastical pressures. John W. Langford and Neil A. Swainson analyze clearly the complex and ambiguous phenomenon of public and quasi-public corporations in British Columbia. Richard Schultz considers one of the most awkward administrative forms, the quasi-departmental regulatory agency, a topic thoroughly familiar to U.S. students of public policy but free of much critical comment in Canada. Particularly significant are the agencies created by Ottawa to increase its control over wheat and petroleum production and over rail transport, all matters of great regional importance in Western Canada. In the best-written and most imaginative piece, "Space, Function, and Interest," A. Paul Pross speculates on the importance of what he calls the special-interest state. Administrative centralization has altered the process of policy formation along sectoral lines, which has thereby helped ensure the decline of political parties as means of communication with, and of legitimization of, government. In this respect, interest groups have partially but badly replaced parties, since, unlike parties, they do not act as brokers for competing interests and are not territorial. One of the responses has been a resurgence of province-building and regionalism.

The next three chapters deal with the problem of bureaucratic accountability. J. R. Mallory offers a thorough analysis of the recent, always difficult, and often futile attempts by the Joint Committee on Statutory Instruments to reformulate the rules governing delegated legislation. O. P. Dwivedi surveys the problem of accountable management within the public service in light of the conflicting doctrine of Ministerial responsibility and the tradition of bureaucratic anonymity. Donald C. Rowat discusses the modest legislative challenges to discretionary secrecy and the degree to which the practice continues in conflict with the democratic expectation that taxpayers have access to official documents they have paid for. The final three essays are not thematically unified. J. W. Grove discusses the dilemmas and impossibilities of attempting to administer that highest, most dangerous, aggressive, and unforgiving of high-tech industries, atomic energy. D. C. Corbett reflects on the similarities of, and differences between, Canadian and Australian administrative practice in the context provided by Taylor Cole's 1949 study of the Canadian bureaucracy and his subsequent work in comparative administration. And finally, R. W. Phidd concludes this worthy tribute to Hodgetts with some pertinent remarks on the lasting conflict of rational, uniform, administrative efficiency with democratic responsiveness, and of participation with governmental responsibility and regional particularism.

American readers may well be surprised at the complacency with which their Canadian colleagues view governmental intrusion into society. This difference in attitude reflects the different orders of size of the two governments as well as a more widespread Canadian sympathy with Fabian views on using the state to enhance social equity rather than seeing it as a threat to liberty. The reason for this, as is hinted throughout this excellent collection, is that in Canada the state has been used less for ensuring market competition along U.S. lines than in maintaining the political integrity of the country. This state-sponsored emphasis on national unity has as often as not been translated into regional resistance to central control. It is a pity this aspect of the administrative state in Canada was ignored.

BARRY COOPER

University of Calgary

Geography and the State: An Essay in Political Geography. By R. J. Johnston. (New York: St. Martin's Press, 1983. Pp. xii + 283. $27.50.)

Both political geography and geopolitics are again drawing some attention, an undoubtedly welcome development because ignorance of geography—of geographical facts, perspectives, and interpretations—has reigned supreme among political scientists for too long. Political geography tends to be regional geography in which the focus is on the state and the impress of political decisions on the landscape. In other words, while in human geography the geographer investigates what humankind has done to its physical geographical environment—how it has been transformed—in political geography the geographer looks specifically at the political person's contribution to the transformation and organization of the earth.

Geopolitics, on the other hand, is not geography, and it may not be even political science (especially if we insist on the science aspect of it). Rather, it is what the etymology of the word suggests: geographical politics. It is neither real geography nor real political science, but rather politics with a geographical perspective (and some consideration of geographical facts). Consequently, geopolitics tends to be prescriptive (and more subjective, ideological, and controversial), while political geography is more descriptive (and acceptable to the academic fraternity).

Although *Geography and the State* is called "an essay in political geography," it is not really that. In fact, Johnston himself says as much when at the very beginning of the Preface he first quotes, obviously approvingly, a statement by P. J. Taylor to the effect that "there can be no distinctive political geographical theory" and "no subdiscipline of political geography," and then goes on to clarify that what he has written "is a contribution to the political geography perspective, not *a* political geography." If the book is not political geography, neither is it geopolitics, although there is a good dose of politics in it. A closer reading reveals an intention to educate on the part of the author. Political scientists may be ignorant of geography, but what bothers Johnston is that his fraternity of geographers is ignorant of politics, the Marxist perspective in politics in particular. His aim is, in the short run, to promote among geographers an understanding of basic political science knowledge and concepts, especially as they concern the state and its subdivisions, and, in the long run, to contribute to the enrichment of the (Marxist) political economy by adding to it a sophisticated political geographical perspective.

As a whole, the book is not of great use to our discipline. Several of its chapters are almost straight political science of a quite elementary character, presumably because it is intended for geographers who have never studied politics. Other chapters focus on the economy, specifically the capitalist economy and its consequences, but here too the facts and the Marxist interpretation are not novel. Even the bibliography, although lengthy, is not helpful because it is neither a basic bibliography in political geography nor a focused specialized one. Probably only the very few political scientists who specialize in the more theoretical aspects of geopolitics will find this book of interest. It is certainly well written, ranges in an informed way over a wide range of topics, and, above all, is a good example of the kind of ferment that goes on in the sister discipline of geography. If we are interested in the geographical perspective in politics, we obviously must know what geography is and what geographers do and think.

LADIS K. D. KRISTOF

Portland State University

Socialism and Abundance: Radical Socialism in the Danish Welfare State. By John Logue. (Minneapolis: University of Minnesota Press, 1982. Pp. xxii + 353. $35.00.)

Scandinavian societies represent to many material abundance and social harmony achieved through the welfare state under the direction of the Social Democratic party. A recent history of material prosperity and a distributive system that aims to minimize economic and social inequality makes an enigma of the "appeal of radical socialism in the democratic welfare state" (p. 6) at the expense of its chief architect.

Perusing the literature for theoretical explanations that might illuminate increased Scandinavian support of radical socialism since the 1960s, Logue identifies and discusses three sets of hypotheses. The first group, "radicalism as rational action," is "based on self-interest in the narrow economic sense" (p. 7), and includes hypotheses on relative deprivation, state of economic development, and the postwelfare society. Logue suggests, with the postwelfare hypothesis, that "though the reduction of economic insecurity has allowed individuals to become increasingly concerned about other issues, the means used to reduce economic insecurity have exacerbated inequality in authority" (p. 12). One would, therefore, expect radical socialism to focus strongly on a redistribution of authority as a political goal. A second group of hypotheses focuses on radicalism as a psychological response (for example, simplicity, authoritarianism, community isolation, and neurosis), while the third group attributes radicalism to political culture.

Armed with potential explanatory variables, Logue chooses the Danish Socialist People's party (SPP) as a "crucial case study," well aware of the limitations and dangers of a single case, but suggests that the results, if not conclusive, are certainly suggestive.

Aware that many readers may not possess expertise on Danish politics, Logue provides a succinct review of the political and economic environment of Denmark. He follows with a synopsis of the history of the Danish Left, from which the SPP emerged in 1958 when a minority of "revisionists" under the leadership of Aksel Larsen was expelled from the Danish Communist party.

Defining the research problem as the determination of "what sort of people voted for the Socialist People's party and why, or, more broadly, asking what sort of voters turned left during the prosperous 1960s and the stagnating 1970s" (p. 153), Logue presents a detailed profile and analysis of the Danish leftist voter in general and the SPP voter in particular.

The study finds no evidence suggesting that the Danish radical socialist voter suffers from absolute or relative deprivation, is geographically, culturally, or socially isolated from the mainstream, is more authoritarian than the average voter, or has a simplistic conceptual framework. Nor is there any indication that radical socialist voting correlates with stages of economic development.

The data do indicate a linkage between radical socialist support and the postwelfare hypothesis, which, as Logue points out, may be spurious due to "a particular pattern of political socialization, or growing up in a working-class, socialist environment with an adulthood characterized by upward mobility through education" (p. 203).

To achieve a more complete picture, Logue eliminates possible "noise of random voting" by focusing only on party members and activists. Analysis of the more limited data set supports the conclusions generated from the analysis of party voters. Among party activists and members, political socialization to radicalism does have impact, but is limited to a relatively small number (approximately 20%) of the party's membership. It appears, however, that class and issue identity rather than party identity is transmitted across generations. The evidence further supports the postwelfare goal of a redistribution of authority as a principle explanation of increased radicalism. Logue concludes, "the Socialist People's Party's relative success has been a product of its ability to combine the traditional socialist appeal for economic equality with a new emphasis on redistributing authority" (p. 301).

This is a carefully designed, well executed, and well-written study on Danish radical socialism that has significance beyond Denmark. It will certainly become reference material for students of Scandinavian politics.

Roger Wall

University of Wisconsin Center/Fond du Lac

The Territorial Dimension in United Kingdom Politics. Edited by Peter Madgwick and Richard Rose. (Atlantic Highlands, N.J.: Humanities Press, 1982. Pp. vi + 256. $37.50.)

In the wake of the United Kingdom's voyage(s) to the brink of devolution and back, Peter Madgwick and Richard Rose have gathered eight essays for a volume intended "to examine collectively the network of institutions that constitutes the United Kingdom" (p. vi) and to assess the U.K.'s capacity to act as a unitary state despite the antics of mischievous nationalists in Scotland and Wales as well as some murderous ones in Northern Ireland. Further, the collected essays purport to illuminate the "territorial dimension" of public policies formulated at Westminster, that is, which aggrieved member of the multinational British state gets what material benefits when and how. This the essays ably do, but not from the perspectives of the "periphery." While Northern Ireland is viewed with ambivalence, the authors otherwise tend to celebrate a centralized government that is "extra good at adaptability, contrivance, elastoplast and improvisation" (p. 98) in the task of keeping the U.K. united via "reform without commitment to fundamental change" (p. 4).

In Part 1, "The Policy Process," the chapters by James G. Kellas and Peter Madgwick on the

Scottish and Welsh Offices in Westminster and by Brian W. Hogwood on the regional dimension of industrial policy both provide useful descriptive excursions into the hearts of administrative darkness within and across the constitutive members of the U.K. These maps of institutional interrelations are most welcome, since Westminister, in response to discontent among the provincials, has confused, if not appeased, them through creating new agencies that intersect with modified older institutions to form a maze of overlapping functions and duplicated efforts. The Scots and Welsh may get their own development agencies, but the purse strings and ultimate policy authority remain in London in the Treasury and in the Department of Industry. In another contribution, Madgwick and Phillip Rawkins reduce the "Welsh Language in the Policy Process" to sentimental sign-post politics. This derisive dismissal may be warranted. However, the only things that do not thematically overlap in these articles are the professed concerns with both the territorial and the functional dimensions of policy. Nowhere in this volume are the economic performances of Scotland, Wales, and Northern Ireland charted in relation to that of England (whose Northern regions are nearly as direly situated in terms of the declining industries and rising unemployment endemic in the "periphery"). What about the possibility of a researchable link between relative regional economic distress and the rise of a language movement? The authors laudably resist single-factor (that is, economic) explanations, but isn't sentimentality (or cultural conceit) likewise a single-factor explanation of nationalism? Since these inquiries are explicitly Westminister-centric, the authors slight questions about the nature of the stimuli to which British governments are responding. In this respect, however, Richard Rose fares better in his analysis of Northern Ireland's anomalous place—the only region with the right to unilateral secession if the citizens so choose—in the U.K.

The four essays comprising Part 2, "Mobilising Popular Support," deserve only scholarly plaudits. In his incisive and insightful article, Jim Bulpitt elucidates Conservative party strategies since 1867 to appease the regions, maintain main-party power, and keep "high politics" as the preserve of the very wealthy. J. Barry Jones and Michael J. Keating explore the effect of regional demands upon the centralist and the decentralist traditions uneasily harbored within the Labour party. They cite the significant implications of devolutionist activity for Labour's electoral strategy, economic policies, and ideology. Ian McAllister compares the Scottish National party, Plaid Cymru, and the Social Democratic and Labour party in terms of their respective capacities to devise plausible short-term transitional policies while awaiting the arrival of their national Godots. Viewed as pressure groups, these nationalist parties are judged to be ironically successful. William L. Miller concludes the volume with an interesting article on the variations in electoral behavior across the U.K.

Although the articles vary in scope and explanatory ambition, the volume is certainly worth perusing.

JOHN KURT JACOBSEN

Rutgers University

The Peruvian Experiment Reconsidered. Edited by Cynthia McClintock and Abraham F. Lowenthal. (Princeton, N.J.: Princeton University Press, 1983. Pp. xxi + 442. $45.00, cloth; $9.95, paper.)

The Peruvian military that stepped confidently into power in 1968 confounded many skeptics by its aggressive steps toward a more independent foreign policy and a stronger state, as well as its sweeping decrees in land reform, management-labor relations, education, the media, and other areas. Yet seven years later, when *The Peruvian Experiment* edited by Abraham Lowenthal (Princeton, 1975) appeared, serious doubts were growing about the regime's viability and the impact of its reforms. In 1980 the military withdrew ignominiously, handing the government back to the same civilian they had overthrown.

In this new book, the result of a workshop held in 1978, aspects of Peru's "Revolutionary Government of the Armed Forces" are analyzed. The quality of the articles is high, and the book provides much useful information. Many of the articles have an analytical basis in international political economy, and some emphasize the impact of more traditional national security concerns (in this case, the possibility of war with Chile after 1973) on government, the economy, and the armed forces as an institution. The corporatist literature, employed extensively in previous analyses of Peru, is almost invisible.

Several debates underlie a general consensus that the revolutionary efforts of the Peruvian armed forces, measured by their actual achievements, largely failed. One divergence is between "structural pessimists" and "policy pessimists"; a second is between "reform pessimists" and "military pessimists." Julio Cotler, Barbara Stallings, Susan Eckstein, and the economists E. V. K. Fitzgerald and Rosemary Thorp consider complications arising from specific policies, programs, and concerns of the military government, yet they stress essentially how historically imposed structural constraints limited the viability of the military's effort from the beginning. Cotler, both a structural and reform pessimist, provides a useful although gloomy overview analysis and summary. In a thought provoking article, Stallings applies various useful analytical distinctions to emphasize that the main influence of foreign capital on Peru was historical—suggesting both the goals of the new government and accounting for many of its constraints—and thus that specific steps by foreign sector actors did not play a major role in determining the fate of the military regime. Eckstein contrasts the results of the military government's efforts from above in different social welfare areas with Mexico, Bolivia, and Cuba's revolutionary experience from below.

Policy pessimists in the book focus on poor national economic policy choices as the primary cause of the government's debacle. Economists Daniel Schydlowsky and Juan Wicht, and John Sheahan, suggest that a focus on industrial export promotion rather than statist import-substituting industrialization could have had a dramatic impact. Sheahan's article is especially interesting, drawing upon the other articles in the book and contrasting the Peruvian case to other Latin American examples. Laura Guasti argues the increased influence gained by international manufacturers and the requirements imposed by the economic model on a poorly prepared government more than offset the declining influence of extractive corporations.

The authors that focus more on the internal political and social dynamics of the attempted reforms diverge over how much importance to place on the military nature of the government in explaining its failure. Cynthia McClintock, Peter S. Cleaves and Henry Pease García, and Liisa North concur that certain characteristics were due to the government's military nature, but emphasize in their essays problems common to all reform efforts in Latin America. Luis Pásara, in an informed article, is a military pessimist who argues that one of the key failures of the regime, its inability to generate popular political support, stemmed in large part from its military character.

Lowenthal, "a counter-vailing optimist," argues the military were in fact largely successful; the country has changed as a result of their rule, in ways they intended and otherwise. In spite of remarkable continuities, "lack of national integration, economic backwardness, badly exploited natural resources, and dependence" (p. 425), power has shifted permanently from the landed oligarchy to new entrepreneurs, to the growing technocracy within the state and outside of it, and to newly mobilized elements of the population, even as the country's economic structure, public administration, and even value structure all have been altered.

The topics Lowenthal's article reviews highlight the fact that overall the book's treatment of issues is imbalanced and incomplete. Notwithstanding variations in nuance and perspective, many articles overlap, while other topics are not covered. Most frustrating is the fact the book barely treats the dynamics of the process of military withdrawal from, and civilian restoration to, government. Indeed, the editors' own assessment that this collection is neither "exhaustive" nor "definitive" is certainly correct (p. xvii). Notwithstanding its disappointments, this book is the best single current overview in English of this critical period. It belongs on the shelf of Latin American and other comparative scholars and could be employed as a supplementary text in certain courses.

JONATHAN HARTLYN

Vanderbilt University

Japan's Economy: Coping with Change in the International Environment. Edited by Daniel I. Okimoto. (Boulder, Colo.: Westview Press, 1982. Pp. xv + 304. $30.00, cloth; $13.50, paper.)

Japan's international role in the 1950s and 1960s was determined largely by its axiomatic relations with the United States. Within the U.S.-dominated and relatively stable environment, the once militarist nation was able to concentrate almost exclusively on economic development. Since the 1970s, however, Japan's economic success, the weakening of U.S. political influence, and disturbances in the global economic system have forced this economic giant to search for a more active role in international politics. *Japan's Economy* examines major economic sources of Japan's search for a new identity.

In chapter 1, "Changes in the International Economic Environment," Hideo Kanemitsu points out that current frictions among the major market economies stem from the tension between free trade and protectionist pressures, the clash between international rules and national policy priorities, and the fundamental conflict between the economic principle of comparative advantage and the political protection of "merit industry." Kanemitsu, an economist, argues that Japan should continue to support the free-trade system because it promotes stable world peace, which in turn best serves Japan's own economic interests. He sees little need for Japan's militarization.

Chapter 2, "Japan and the World Energy

Problem" by Herbert I. Goodman, describes Japan's efforts to diversify sources and types of its energy supplies and concludes that despite such efforts, Japan's and other countries' dependence on the Middle East oil will continue for quite some time. Goodman does not make clear how Japan should or can generate the necessary political influence in creating a stable international environment and controlling energy supply disturbances. Limitations of purely economic analysis also become apparent in chapter 3, "Internationalizing Japan's Financial System" by Eric W. Hayden. Hayden, an economist, describes the trends since the 1970s toward liberalization of Japan's domestic financial system and internationalization of the yen. He sees in those trends Tokyo's potential as a major international financial center. But does Japan have the will and ability to carry the political burdens usually associated with a major international financial power? The question remains unanswered.

Chapters 4 through 7, titled respectively "Cyclical and Macrostructural Issues in U.S.-Japan Economic Relations" (by Gary R. Saxonhouse); "The Economic Dimensions of the U.S.-Japan Alliance: An Overview" (by Hugh T. Patrick); "The U.S.-Japan Trade Conflict: An Economist's View from Japan" (by Ryutaro Komiya); and "The Economics of National Defense" (by Okimoto), focus on U.S.-Japanese relations and explain the economic sources of recent and current bilateral frictions.

Saxonhouse and Komiya warn that the commonly held assumption that synchronization of U.S. and Japanese economic policies will serve their mutual interests ignores the fact that such "coordination," even if possible, may destabilize other parts of the international economic system. Patrick and Komiya share the view that U.S. preoccupation with bilateral trade disequilibria is not only ineffective in managing the long-term bilateral economic relations, but also harmful because what matters are not merchandise trade imbalances but current account deficits that result from multilateral trade and monetary transactions and which are to a large extent beyond the control of the two governments involved. The three economists agree that greater political tolerance must be cultivated toward short-term trade imbalances and fiscal and monetary policy differences between the two governments. Clearly more dialogue is urgently needed between political science and economics.

This need is clearly demonstrated in the last substantive chapter in the present book. Citing the results of a simulation by Hugh Patrick and Henry Rosovsky ("Japan's Economic Performance," in Patrick and Rosovsky (eds.), *Asia's New Giant,* Brookings Institution, 1976) and reporting his own simulation results, Okimoto points out that a major transfer of capital from private and public investments to defense programs in Japan, constituting an increase of defense spending from less than 1% to 2% of GNP, would result in only temporary, short-term declines in economic performance indicators and produce substantial improvements thereafter, surpassing levels achievable without such capital transfer. Okimoto concludes from this that while in the earlier decades the "free-rider" criticism against Japan may have been accurate, such no longer appears to be the case. Okimoto does recognize the political costs involved in substantial increases in defense spending.

In short, despite the limitations of the economic analyses reviewed here, *Japan's Economy* poses a serious challenge to political scientists who are concerned about the management of future political problems caused by economic developments in Japan and elsewhere.

TSUNEO AKAHA

Bowling Green State University

Contemporary Democracies: Participation, Stability and Violence. By G. Bingham Powell, Jr. (Cambridge, Mass.: Harvard University Press, 1982. Pp. 279. $25.00.)

"Why does the political process work more successfuly in some democracies than in others?" (p. 1). Analysts of the public choice tradition assume that democracy is an abstract mechanism of social choice. They achieve theoretical elegance by conducting a micro-level examination of alternative voting schemes on such well-defined desirable traits as transitivity of group preferences. The narrow questions, however, often beget acontextual and ahistorical analyses. Analysts of the aggregate data tradition, on the other hand, assume that democracy is a concrete system of governing. They achieve contextual richness by conducting a macro-level examination of such broad desired traits as institutional performance. The broad concerns, however, often beget empirical eclecticism.

Powell's work is one of the very best examples of the aggregate data approach. Arguments from the literature about democratic political performance are summarized and explored, with measures aggregated at the nation-state level for a cross-section of 29 contemporary democracies between 1958 and 1976. Powell's organization is a model for such studies. He first examines the impact of sets of independent variables, the social and economic environment, the constitutional setting and party systems, on all performance (dependent) variables. He then proposes process models for each dependent variable: citizen electoral participation, executive and governmental stability, and political violence.

The substantive and theoretical sophistication of the data analysis makes Powell's work one of the very best exercises in the field. Powell collected much of the data himself, and his work paid off. He is sensitive to the nuances of theoretical arguments, aggregate data, and specific cases. For example, his exploration of ethnic rioting (pp. 46-47) shows great insight. His arguments about the role that political parties play in violence and regime change (p. 168) are interesting and informative. Even when cross-national data is not directly adduced to probe arguments, Powell offers fascinating insights into some of the following key issues in Western politics: abortion laws, civil liberties, registration laws, agricultural minorities, income inequality, manipulation of the economic cycle, openness of the economy to internal trade, federalism, coups, and capitalism.

There is thus one problem with the work: Many seemingly unrelated arguments are tested. Powell's models are a complex mix of positive and negative, intervening and controlling, direct and indirect, as well as cost and benefit variables. The sample consists of 29 countries, but the tables list approximately 66 different variables and 460 different correlation/regression/factor analysis coefficients. The number of other data analytic comparisons (for example, percentages, medians) is almost as great. Moreover, the number of variables discussed but not explored empirically is also great. If the advantage of comparative over case studies is to reduce the ratio of variables to cases, then such an exercise cannot be considered progress. Powell provides descriptive models, not parsimonious explanations.

The real problem is that Powell does not consistently pursue a unifying theme that integrates the analysis. There is a focus on the central role of political parties as linkages between the social and economic environment, the constitutional setting and political performance. But this is the book's broad topic rather than its central argument. At bottom, Powell tests theories of why coalitions coalesce, protesters protest, and voters vote. Thus, the concluding discussion of "leadership and political creativity" is again fascinating, but Powell's strength is really his undoing: He achieves contextual richness at the cost of some empirical eclecticism.

In summary, Powell's work is of great breadth and depth and will become the standard of a certain genre of research. It is undoubtedly the best empirical collation and test of partial, middle-range theories of macro-level performance in democracies yet done. All students of Western-style democracies and of political performance will benefit from reading Powell's work more than once. The problems with his analysis are really limitations of the genre of research. Aggregate data studies of political performance do not have the theoretical boldness of many verbal theories of performance. Nor do such studies have the deductive elegance of the public choice theorists' analyses of democracy. Rather, such studies, when well done, offer theoretical comprehensiveness and contextual richness. In this sense, I have not criticized the artist so much as the state of the art.

MARK LICHBACH

University of Illinois at Chicago

Women and Politics. By Vicky Randall. (New York: St. Martin's Press. Pp. 227. $25.00.)

As an introduction to basic issues involved in a study of women and politics, this book provides a well-written survey of the growing body of literature and theories. Randall attributes the subordinate place of women in the political structure and, until recently, relative lack of involvement in politics as consequences of prior definitions (developed by men) of women's innate physical inferiority and/or submissive social roles. The combination of concern with a problem—women's lack of involvement in the political sphere—and concern with the cause—male domination—results in a "feminist perspective" seeking to both understand the cause and correction of the problem.

Indeed, in the Introduction Randall discusses "What is Feminism?' and in chapter 1, "Women's Place in Society," reviews theories documenting the fact of "male dominance and patriarchy" even while asking if such arrangements between the sexes are "inevitable." What follows is an assessment of women's participation in fundamental types of political involvement—such as participation as citizens, participation as office-holding political elites, participation as members and leaders of political and social movements—as well as the policy consequences accruing to women from each type of political activity. This scope includes a useful cross-national and comparative perspective in the context of discussions about "How Politics Affects Women" (chap. 4) and "The Politics of the Women's Movement" (chap. 5).

It is refreshing to find a treatment of such a wide range of topics from the perspective of a single author versus the more usual edited anthologies. Furthermore, Randall attentively cites contributions to what is the developing canon of literature on women and politics. In addition, Randall's style is lucid and careful; she is aware of the complex arguments and divergent points of view entailed in the subjects that she discusses, making this book both a good introduction and a guide for further study.

If there are exceptions to the book's merits, they are the other side of its strengths. The commendable breadth of topics included almost necessitates that some subjects either will be omitted or gone over too lightly. So, for example, "woman suffrage" is missing from the index and treated summarily in the text. However, many would point to this battle—staged internationally and with particular intensity in the United States and Britain—as the focal point for women's rights movements seeking redress for male-induced subordination identified at the book's outset as the cause of women's political problems. Similarly, "constitutional law" is not indexed. Rather, "law" is subsumed under "policy" concerns, despite the unusually rich opportunities afforded by this topic to document the "intellectual mentality" rationalizing male-dominated institutions as well as the rate and way in which this mentality slowly changed.

Although Randall is candid about presenting no "single, unified interpretation of the relationship between women and politics" (p. 199), nevertheless, an implicit utilitarianism runs throughout. It is perhaps most evident in such statements: "It only really makes sense to emphasize women's participation in public policies if it can be shown that policy outcomes do affect women" (p. 107). Randall's passive theoretical orientation may account for the failure to explicitly develop either the conceptualization of women as individuals possessing natural rights demanding democratic recognition or as group members joining pluralistic frays in democratic competition for political goods. Both premises are integral to democratic theory and practice; the book's theoretical focus could have been enriched by reference to classic democratic formulations as applied to the analysis of women's increasing integration into the political sphere.

With these reservations aside, *Women and Politics* is recommended as a thoughtful starting point for students interested in reckoning with the issue of women and politics.

EILEEN L. MCDONAGH

Northeastern University

Political Legitimation in Communist States. Edited by T. H. Rigby and Ferenc Feher. (New York: St. Martin's Press, 1982. Pp. xiii + 177. $22.50.)

More and more, students of the Soviet-type communist states are joining the mainstream of comparative politics by discussing the "legitimacy crisis" in these political systems. In an effort to go beyond commonsense observations, a group of scholars was assembled in mid-1979 at the Australian National University to analyze the topic in a more serious way. This volume, the first on the subject, resulted from that meeting. Whether it contributes to, rather than dissipates, the confusion in the current discussion of legitimacy is a question wisely left open by the editors themselves in their preface.

Despite the good intentions of its nine authors to bring the study of communist countries closer to the heart of the discipline and to break new ground, the book is a disappointment. Perhaps it is too early in the exploration of the topic for a symposium such as this.

The disappointment derives from the uneven quality of this book's contributions. Rigby provides an excellent introduction, the most thoroughly documented chapter of all. It is really the basis of a monograph in its own right which one wishes had been developed. Unfortunately, this good beginning is not followed through by the other authors. Only Robert F. Miller's chapter on the Eurocommunist parties' search for legitimacy and that of R. N. Berki on the philosophical basis for legitimation (or rather the lack of it) in Marxist-Leninist states are cogent, interesting, and pertinent. Of the remainder, the most common weakness is the failure to define terms and to articulate clearly at the outset a theoretically relevant problem. This, together with what in several instances appears to be rather skimpy research (one chapter has 10 end-notes in which are cited only two actual sources), results in unfocused discussion consisting of generalizations as seemingly endless as they are groundless. This equation of political science with sweeping historical generalization apparently stems from the East European training of some of the authors. Readers of this journal may wish for a bit more of the rigor associated with the discipline in its predominantly American mode.

It is regrettable that, apart from quality of research, none of his fellow authors follows Rigby's conceptual lead either. None takes up his definition of "legitimacy," nor his approach to legitimation as a process, nor any of the hypotheses derivable from his careful exegesis of Max Weber's writing on the subject. Therefore, this is not a collective enterprise, but rather, with the exceptions noted, a collection of individual discussions of various aspects of legitimacy and legitimation. It tantalizes without satisfying. Nevertheless, it will be useful in graduate seminars where new directions of research on communist countries of the Soviet type are being probed, because it does at least clear the underbrush so that further research can proceed.

BOHDAN HARASYMIW

University of Calgary

Latin American Prospects for the 1980s: Equity, Democratization, and Development. Edited by Archibald R. M. Ritter and David H. Pollock. (New York: Praeger Publishers, 1983. Pp. ix + 330. $29.95.)

This is a Canadian product, based on the proceedings of a conference held in 1980 in Ottawa. The authors include six Canadians and six Latin Americans, mostly academics, economists, and political scientists, and three either with, or formerly with, the Economic Commission for Latin America, and one Minister of External Relations (Trinidad/Tobago). No Canadian officials participated, and although much is made of the interplay between U.S. policy and Latin American internal affairs, Canadian policy is not discussed.

The conference and the volume came 10 years after an earlier effort that focused on revolutionary change. The central word in the present volume is "equity." Although there is no attempt to fit the separate chapters into a rigorous analytic scheme, there seems to be a largely shared outlook. Inequality in Latin America is both a historical given that helps account for contemporary dialectics and a continuing, indeed worsening, moral and policy failure. Cuba is not placed on a pedestal, but only Trinidad/Tobago's External Minister criticizes Nicaragua and Grenada. There is no contributor who describes any Latin American country as "merely authoritarian." As one contributor puts it, the Kennedy approach to Latin America was dead by the end of the Carter presidency, and the election of Reagan is a relapse to Eisenhower and before.

While the conference was held in 1980, the resulting papers do not seem dated. Some of them were apparently finished in 1981, and events in Latin America since then have not been marked by sharp change. The authors were not sanguine about the military's yielding power in Brazil or Chile or the dismantling of their repressive apparatuses. If the world recession has been worse than foreseen, then the repression needed to make orthodox economic policies stick in the face of worsening unemployment and greater inequity can hardly be less. Of the various contributions, only Claudio Orrego Vicuna's on human rights is optimistic. He feels that the authoritarian regimes are not mellowing, but that human rights is a movement gathering moral force among individuals and organizations.

This is the most explicitly normative of the 13 contributions—hopeful of a broad coalition from Christian to Marxist and mindful of the need "to define an institutional role for the armed forces that is compatible with democracy and humanism" (p. 52). The other contributions are more descriptive or analytical. The Canadian contributions tend toward the general and analytic, the Latin American toward description and interpretation of their countries of expertise.

Among the general, Albert Berry, in "Predicting Income Distribution in Latin America during the 1980s," does not foresee that labor will become sufficiently scarce to overcome the greater inequality produced by education differentials. Inequality enters Alan B. Simmon's contribution more obliquely. He writes on the need to modify the classical view of the demographic transition accompanying development to fit Latin American (and perhaps Asian and African) realities. Preexisting inequality in land, income, and influence, along with public health measures unrelated to level of development, lead to policies that push excess rural labor into the cities at low levels of industrialization.

Myron J. Frankman looks at the postindustrial service sector in the advanced countries and concludes that there are advantages in being a latecomer. He recommends that "we relieve industrial expansion of the principal responsibility for resolving the employment problem" and create jobs to "increase the enjoyment of life" in education, health, and nutrition, but notes that "advice from the IMF" is at "cross-purposes" with government-budget growth (p. 143).

Among the country-specific chapters, Archibald R. M. Ritter examines Cuba's new, ostensibly democratic, state institutions and finds the Party still dominant. Maria Helena Moreira Alves gives a detailed account of the "Mechanisms of Social Control of Military Governments in Brazil, 1964-80," and Jorge Nef connects "Economic Liberalism and Political Repression in Chile." The chapters on Peru and Ecuador, relative success stories for democracy, display only guarded optimism.

We're not in for an easy decade. As one of the editors concludes, the central question is whether "distributional equity" will be effected by a "strategy of rapid growth cum incrementalism or by a political strategy of violent paroxism" (p. 20).

ELISHA GREIFER

Northern Michigan University

Fiscal Stress in Cities. Edited by Richard Rose and Edward Page. (New York: Cambridge University Press, 1983. Pp. viii + 245. $29.95.)

Fiscal Stress in Cities is a collection of papers presented at a conference on urban fiscal problems in Great Britain. Since the book is about a country with a very different structure of local government than ours and contains very little explicit comparative material, I feared that I would have no framework with which to interpret it. However, the book is highly readable, it translates well, the essays are of high quality, and are well selected. More importantly, the book offers some new concepts, deals with important theoretical issues, and poses some interesting policy issues. It deserves a wide reading.

The essays are well laid out, moving from descriptions of the problem in Great Britain to analysis and theoretical conclusions. Thus the essays describe the effects of national economic policy on urban finance, the central government role in creating local fiscal problems, school finance, and labor problems, and then move on to discuss the issues of reduction of local autonomy and the demise of incremental budgeting. The reader will be struck by the similarity of problems facing British and American cities. It is fascinating to watch another nation wrestle with many of the same problems in a different structure and culture.

One theme moves through the essays, giving them a unity usually lacking in published symposia. The theme is that national economic policy and local fiscal problems are interdependent. The national government has determined to reduce the proportion of the economy absorbed by the public sector, in an effort to increase productivity, reduce debt, and reduce interest rates. As part of this policy, the national government has created a policy of reducing local expenditures and in the process has curtailed local autonomy. (This part is quite different from policy in the United States.) As different instruments were deployed to curtail local expenditures, the result has been an exacerbation of uncertainty at the local level. Local budgets have varied from year to year over the past decade in a manner that Richard Rose and Edward Page compare to a "random walk."

The theoretical conclusions of the book can be summarized in two phrases: Governments cannot control themselves, and the cutback process creates continuing financial instability. Governments cannot control themselves because they are open to international economic and monetary systems that bring about continuing changes to which national economic policy must respond. These policy changes in turn bring about chronic financial uncertainty at the local level, which results in local level coping strategies instead of management and planning strategies.

The national-level policies have markedly reduced local autonomy, an issue that has been hotly debated in Britain, and which is reflected in the book. The authors agree that local autonomy has been reduced but disagree as to the cause and the importance of this change. While these key questions are not satisfactorily answered in this book, they are raised in a most provocative way.

IRENE RUBIN

Northern Illinois University

From Hitler to Ulbricht: The Communist Reconstruction of East Germany 1945-46. By Gregory W. Sandford. (Princeton, N.J.: Princeton University Press, 1983. Pp. xiv + 313. $25.00.)

The collapse of Hitler's Third Reich in 1945 left Germany divided into four zones of occupation (plus Berlin) and deprived of its eastern provinces. Subsequent disputes among the wartime Allies rigidified the country's division, and by 1949 the Federal Republic of Germany (FRG) had been created in the West and the German Democratic Republic (GDR) in the East.

Western scholarship outside the Federal Republic has by and large slighted the emergence and development of the GDR. In part this is due to the West's commitment to and fascination with the FRG. The very nature of the ideological struggle between East and West made it difficult for writers to avoid polemics; and "scholarship" all too frequently ranged between expressions of sympathy for East Germans caught under the Soviet thumb and condemnation of the GDR's illiberal regime. In part, too, the closed nature of East German society made it difficult for scholars to obtain the information required for balanced judgments.

Although one can point to earlier landmarks, many of them translations of West German books, it is only recently that English-language studies of the GDR have begun to appear in sufficient number to give us a clear idea of what has happened and is now happening in East Germany. It is in this sense that Sandford's account of the country's earliest days makes its most important contribution.

The first third of the book provides an excellent summary of developments leading up to and including the Potsdam Conference of July and August 1945. Sandford details the love-hate relationship between social democrats and communists during the 1930s and early 1940s, wartime efforts by the Soviet Union to position itself and its German emigrés for the eventual occupation of Germany, and early postwar activity on the part of Soviet officials to restore order while at the same time ensuring that their vision of a democratized Germany would prevail throughout the land.

Subsequent chapters, focusing on land reform, institution building, and expropriation of major industries, comprise the heart of the volume. Although readers thoroughly familiar with the German-language literature will find in them no startlingly new information or interpretations, these chapters are distinguished by cogent presentations based in part on interviews conducted in the GDR and material (such as doctoral dissertations prepared for East German universities) that is not generally available in the West. Sandford is thus able to provide rich detail to enhance our understanding of how the future GDR's leaders, men such as Walter Ulbricht and Wilhelm Pieck, worked together with Soviet officials to transform that portion of German society and economics under their control. By summer 1946, he concludes, the Soviet zone of Germany "was already the 'democratic people's republic' of prewar Communist theory."

The volume belongs on the shelf of anyone seriously interested in postwar Germany or the emergence of the Soviet bloc. This is not to say that it is without problems. Sandford tends to slight relevant developments in the West, denigrate the genuine accomplishments of the Western Allies in "democratizing" their zones of Germany, and underplay the awe-inspiring influence of the Red Army in persuading East Germans to go along with Ulbricht's plans; he slips too easily into a descriptive mode, substituting sequences of events for analyses of how they came about; and one might wish that he had devoted more attention to the creation of party and governmental institutions (which, it must be added, have been solidly treated elsewhere, e.g., Henry Krisch's *German Politics Under Soviet Occupation* [Columbia University Press, 1974]). It is nonetheless a vivid and valuable account of how a part of Germany was put on the path toward the Soviet brand of socialism.

RICHARD L. MERRITT

University of Illinois at Urbana-Champaign

Managing Public Enterprises. Edited by W. T. Stanbury and Fred Thompson. (New York: Praeger Publishers, 1982. Pp. xii + 301. $29.95.)

The 13 articles comprising this book were selected from papers presented in 1981 at two conferences. Although a majority of the authors are economists, the papers vary widely in focus and approach. Some of the articles are narrowly focused or highly technical, while others take a broader perspective that is more likely to be of interest to political scientists in this area. The volume is divided into three parts: "The Purposes of Public Enterprises," "The Performance of Public Enterprises," and "Management Strategy of Public Enterprises."

The first section, on the purposes of public enterprises, contains two useful essays. Arie Y. Lewin briefly examines the significance and performance of state-owned enterprises in the countries of Western Europe, where such enterprises commonly dominate strategic sectors such as banking, communications, airlines, and oil. Lewin emphasizes that most state-owned enterprises are torn between the economic goal of profit maximization and the often conflicting social goals that initially prompted national control of the enterprise. Larry Pratt's "Oil and State Enterprises: Assessing Petro-Canada" is one of the few selections in the book that explicitly addresses political questions. Pratt argues that a national oil company can serve a number of important political purposes, including securing energy supplies, securing control over the economy, securing governmental revenue, and securing opportunities for domestic capital. Using Petro-Canada as an example, Pratt finds the losses of economic efficiency that are involved in establishing a national oil company to be minor and well worth the political gains. This is the most explicitly political of all the essays in the book and provides a useful counterweight to the exclusive focus on economic efficiency that underlies many of the other writings.

Part 2 considers the performance of public enterprises. Several of the essays in this section carefully compare the economic efficiency of public versus private enterprises by examining longitudinal data on the performance of Canadian, American, and Australian railroads and airlines. These studies indicate that the form of ownership has little effect on organizational efficiency (as measured, for example, by full factor productivity). The major determinant of efficiency is not mode of ownership, but degree of regulation. A publicly owned enterprise that is able to respond to competitive market forces with little regulatory intervention is likely to be far more efficient than a private enterprise that is heavily regulated. Two other essays in this section are of interest. George W. Hilton briefly discusses the conflicting demands that have impaired the performance of Amtrak. In the only theoretical essay in this section, Catherine Eckel and Aidan Vining use public choice theory to suggest the advantages of joint private-government ownership of some enterprises.

Part 3, on strategic planning by public enterprises, is composed of generally uncritical case studies. Accordingly, it lacks the broader relevance of the other two sections. An exception is an article by Christopher H. Lovelock and Charles B. Weinberg that examines the marketing strategy of U.S. Postal Service. The authors argue that the Postal Service's neglect of marketing has had substantial costs. They suggest political and other causes for the neglect of marketing and advocate remedial steps. Although a case study, the article's analysis can be generalized to the marketing (and strategic planning) problems of many public enterprises.

In sum, political scientists concerned with such topics as government corporations, regulatory reform, or public versus private organizational efficiency are likely to find several interesting articles in this volume.

JAMES E. SWISS

North Carolina State University

South Yemen: A Marxist Republic in Arabia. By Robert W. Stookey. (Boulder, Colo.: Westview Press, 1982. Pp. ix + 124. $16.50.)

This very short and disappointing book is the only one available in English on contemporary South Yemen, officially the People's Democratic Republic of Yemen (PDRY). The book has two declared goals: (1) to provide the general reader with a succinct description and balanced general introduction to the history and culture as well as the contemporary politics and economics of South Yemen; and (2) to explain how and why an authentically Marxist regime, rather than a more conventionally heterogeneous nationalist one, emerged immediately out of the colonial experience in this small country on a far corner of Arabia.

Stookey achieves the first goal far more successfully than the second. The book is a well-written, interesting, and informative introduction. Chapter 1 provides a good sketch of the land and people and is notable for a succinct discussion of the traditional social structure and its origins. Chapter 2 ably leads the reader through South Arabia's rich and complex history from antiquity until the British occupation of Aden in 1839, and chapter 3 does the same for the century-and-a-quarter of colonial rule that ended with independence in 1967. Chapters 4 through 6, dealing with the society and economy, political system, and international relations of the PDRY since 1967, present a balanced and at times sympathetic treatment of the PDRY's Marxist experiment. The regime is given credit for its dedication to its chosen path and for its achievements in political organization and in creating a more egalitarian society through education, income distribution, and women's rights.

I have only a few criticisms of this book as a general introduction. A good short book requires the rigorous application of a standard of inclusion and exclusion, and Stookey sometimes fails this test. For example, he lists some 32 birds and animals found in South Yemen but neglects to give its human population. More important, his information on, and analyses of, politics, public policy, and international relations becomes extremely thin as he moves from 1967 through 1980—much thinner than the available materials warrant. Unfortunately, this is probably the period of greatest interest to this book's potential readership.

Stookey's attempt to explain the how and why of a Marxist regime emerging immediately out of colonialism falls far short of the goal. His answer to this intriguing question is too sketchy and fragmentary to be convincing. The core of his explanation is that the colonial rulers unwittingly disrupted the traditional social system and dispossessed a large part of the population, thereby creating fertile soil for social revolution instead of merely national liberation. As a consequence, the ideology of the Arab Nationalist Movement, which in the 1960s migrated south from Beirut to Arabia and left from nationalism to Marxism, took root and flourished in this unlikely place. Stookey may have most of the answer right, but for me to be convinced, this answer would have to be elaborated and supported with factual detail. He does not tell me what made South Yemen's experience with colonialism so different from that of the many third-world nations that began independence with a more conventional nationalist phase.

Stookey, a former foreign service officer who served in North Yemen in the early 1960s and the author of by far the best scholarly study of the politics of North Yemen, is well qualified to undertake an analysis of the politics of the PDRY and this just adds to my disappointment. It would seem likely that there are far fewer general readers eager for an introduction to South Yemen than there are area specialists and political scientists interested in a detailed and well-reasoned analysis of the how and why of the PDRY.

ROBERT BURROWES

New York City

Administrative Systems Abroad. Edited by Krishna K. Tummala. (Washington, D.C.: University Press of America, 1982. Pp. viii + 378. $25.50, cloth; $14.00, paper.)

This is primarily a collection of case studies of national public bureaucracies in the Third World that is weighted toward Asia. Its strength lies in the authors' detailed knowledge of individual countries. Its weakness lies in the relative absence of systematic comparisons and in the extent to which the literature of related fields is ignored.

In the one African case, Peter Koehn examines Nigerian public administration. It is a story of progressive deviations from the British model in the context of a regionally based federal system. David Rosenbloom and Gregory Mahler survey the multiple problems of Israeli administration and tangentially discuss the distribution of ministries in coalition governments. Anne Freedman and Maria Chan Morgan contribute a cogent analysis of the political and "rational" means employed to control bureaucracy in the People's Republic of China from 1949 to 1980. Atilla Dicle paints an interesting but primarily historical picture of administrative arrangements in Turkey.

In the first of three essays on the Indian subcontinent, Krishna Tummala writes, rather opaquely, on the "Higher Civil Service" in India. Robert LaPorte, Jr. provides a successful account of unsuccessful administrative reforms in the context of military rule in Pakistan. Mohammad Mohabbat Kahn and Habib Mohammad Zafarullah complete the trilogy with their description of a "denounced, demoralized and demotivated" postindependence bureaucracy in Bangladesh.

Jon Quah's clearly written study of public administration and national development in Singapore is refreshingly upbeat. It is a success story of a bureaucracy that has managed to maintain high personnel standards, control corruption, and implement substantial socioeconomic development programs. Linda Richter has less good news to report in writing of "bureaucracy by decree" in the Philippines. It is an account of intelligent people being coopted into a corrupt system. In a study of Indonesia, Garth Jones describes the interesting practice of appointing junior cabinet ministers to oversee task-oriented programs requiring extensive coordination, although we are not told how successful this arrangement has been.

Latin America is left to John Sloan, who contributes a stimulating comparative analysis based upon the premise that Latin policymakers have generally stressed bureaucracy over social mobilization, leading to the bureaucratic authoritarian option. However, more could be said about the relevance of international bureaucracies, the impact of economic developments, and the socialist experience of Cuba, Nicaragua, and Allende's Chile.

The book also contains a short prologue by Tummala, a very general article on comparative bureaucracy by Gerald Caiden, and best of all, a review article by Fred Riggs. In the absence of a general organizational framework, the Riggs article pulls the book together. With one or two exceptions, the authors of the case studies consider only one country each and fail to incorporate relevant materials from the voluminous literatures of comparative politics, organization theory, contemporary development administration, or from the general works on the military in politics. They are thus unable to account for administrative variations between countries. Riggs demonstrates a broader perspective in arguing that the difference between Bangladesh and Singapore is largely that an extrabureaucratic political structure is dominant in Singapore. The fundamental determinants of bureaucratic behavior are thus political, although one must understand that civilian/military bureaucracies are inseparable parts of political systems. Some of Riggs's arguments are debatable, for example the claim that presidential systems of government tend to lead to military dictatorships and autocracy (p. 366). Nevertheless, he ranges beyond the other authors in stressing the importance of technological and world-system contexts and concludes with a discussion of the fate of comparative administration, the field he was instrumental in founding.

RICHARD E. HARTWIG

Illinois State University

Tanzania: An African Experiment. By Rodger Yeager. (Boulder, Colo.: Westview Press, 1982. Pp. xii + 136. $18.50.)

Tanzania is the second country report in the new Westview Press "Profiles" series, "Nations of Contemporary Africa." Despite its remoteness and poverty, the politics of Tanzania remain a growth industry, reflected in more than 20 years of commentary, including secondhand case histories in introductory comparative politics text books. At the very least, Rodger Yeager's short but informative case study provides a balanced and yet challenging introduction to a new generation of students who may be puzzled over what the fuss has been about.

Yeager, who now teaches at West Virginia, has been intimately involved in Tanzanian affairs since the mid-1960s, having worked there as researcher and Tanzanian government adviser. His approach focuses on the political economy of development, combining historical description with a concern for macropolicy questions. While clearly sympathetic to the Tanzanian "experiment," Yeager does not eschew criticism of policy errors. Readers will probably conclude that Tanzania is neither "a model of modernizing achievement" nor "a lesson in development failure" (the evaluative theme of the study, first noted on page 3); but they undoubtedly will be fascinated by the efforts of the country's leadership to create a society of justice and well-being in a hostile environment.

The analytical thread of choice and the cost of alternatives runs through the whole volume. In the early 1960s it was nonracialism rather than partnership or racism, confederation with Zanzibar rather than rejection or take-over, one party rule with participative institutions rather than *personalismo,* and a nonaligned foreign policy. In company with other observers Yeager emphasizes the key role of Nyerere as an ideological guide and as political tactician. The major policy choice of the country centers on the Arusha Declaration and the politics of more centralized socialism of the late 1960s and 1970s. Nyerere himself summarized the price paid in economic declines traced to bureaucratized administration and failures of collectivized agriculture—essentially the cost of a more equitable society that has achieved a certain minimum welfare standard.

Despite internal crises of production and administration, Tanzania has continued to choose, almost perversely, an active and independent foreign policy in the last decade in the face of international opposition. "Self-Reliance" as a development policy has meant the unravelling of older skeins of economic cooperation in East Africa. Yet the leadership pursues new attempts in southern Africa, at great risk, related to Tanzania's position as a front-line state in the struggle against apartheid in South Africa. Assuming great economic and logistical burdens, Tanzania undertook the overthrow of the Amin regime in Uganda and played an important role in the restoration of President Obote. Although accepting military assistance from Communist governments, Tanzania still receives most of its economic aid from the West. To deal with the present crisis of finance and possible famine, the country must negotiate major readjustments in the economy to satisfy international lending agencies.

Yeager concludes by summarizing the criticism Tanzania has incurred from scholars of the Right and the Left. Consonant with the continuing debate over socialist policies the world over, Tanzania is cited as typical of the inherent problems of a socialist development program, while it is also castigated as weakly populist in its socialism, not radical (class-war prone) enough. Nevertheless, as Yeager makes clear, the final choice may not be between forms of socialism. It is between some form of bureaucratic-patrimonial authoritarianism and a participant and humane system that survives the departure of Nyerere as leader.

Replete with evocative photographs and a useful bibliography, the book is an auspicious beginning of what ought to be a valuable series. It deserves a paperback edition to make it more widely available.

HARVEY GLICKMAN

Haverford College

The Political Economy of Nigeria. Edited by I. William Zartman. (New York: Praeger Publishers, 1983. Pp. vi + 283. $30.95, cloth; $13.95, paper.)

This collection of seven essays deals with the political economy of the second Nigerian republic. Although all the essays are of high quality, they differ in voice, methodology, and focus. What unites them is a shared concern for a common set of problems: the interrelationship between modernization or capitalism, the formation of classes and ethnic groups, and the role of politics and the state.

Zartman and Sayre Schatz write the Introduction. Larry Diamond has an intriguing chapter relating ethnic and class formation to political competition in the Second Republic. Henry Bienen writes on income distribution and politics and disagrees with Diamond that class has replaced ethnicity as the major line of cleavage in Nigerian politics. Michael Watts and Paul Lubeck explore the economic and political effects of the oil boom. Adeoye A. Akinsanya examines government policies toward Nigerian and foreign business. Thomas Bierstekker follows on Akinsanya's efforts by focusing more closely on the policy of indigenization and the government's attempt to control transnational corporations. Timothy M. Shaw discusses Nigeria in the international system. By this he means to include not only the conventional issue of inter-state relations, but also her economic relations as a country on the "semi-periphery" of the world capitalist system. This set of essays is complemented by an annotated bibliography compiled by Robert Mandersheid.

Although neither Diamond nor Watts and Lubeck make the elementary error of confusing ethnicity with traditional precolonial solidarities, and they do suggest that ethnicity still plays a role in certain cases, the thrust of their argument is that ethnicity and class are at odds with each other and that, at least in Nigeria, class is winning. They cite the realignment of the ethnic basis of the major national parties and the class struggle in the North, which has at times taken violent if not millenerian aspects.

Reexamining the evidence, however, it would seem that final rites over ethnicity in Nigeria and elsewhere are somewhat premature. Although it is true that the National Party of Nigeria (NPN) has been able to create an electoral coalition cutting across ethnic segments, other parties such as Awolowo's United Party of Nigeria (UPN) have been less successful. Indeed, the very success of the NPN may demonstrate neither the increased significance of class issues nor the relative insignificance of ethnic issues. It does demonstrate that constitutional engineering, namely the creation of more states, has produced opportunities for cross-ethnic alliances. Indeed, it could be argued that the pattern of alignments in the NPN is testimony to the endurance not only of ethnicity but also of old ethnicity deriving from the First Republic. As to class struggle in the North, that too is not a new phenomenon. It was said in the First Republic that if a class-based revolution ever manifested itself, it would commence in the North.

The point of these comments is not to minimize class as a significant cleavage in Nigeria. What is suggested is that class and ethnicity not be viewed as polar opposites with the one "ineluctably" replacing the other. In culturally plural societies such as Nigeria, India, Canada, and the Netherlands, class and ethnicity can be complementary orientations.

Beyond addressing themselves to the ethnic-class controversy, these essays do raise genuinely new and interesting problems. In particular, Diamond calls attention to political intolerance as a worrisome and persistent feature of Nigerian democratic politics. It is this intolerance, a mixture of chicanery, ruthlessness, and refusal to play by the rules, which even more than ethnicity and class conflict constitutes a great danger to Nigerian democracy. Diamond suggests that this unwelcome side of Nigerian politics is not a product of political culture, but is a byproduct of a dominant and centralizing state, swollen with oil revenues. He proposes that in the absence of an active and indigenous capitalist sector, state power becomes the "only game in town." Thus, a fundamental problem in Nigeria today is that a free and plural politics leads to a mad scramble for the resources of a directed and centralized economy. It may be that to make democracy work in Nigeria, what is needed is not only more states but also more centers of economic opportunity and power.

ROBERT MELSON

Purdue University

International Relations

The Nuclear Arms Race: Control or Catastrophe? Edited by Frank Barnaby and Geoffrey Thomas. (New York: St. Martin's Press, 1982. Pp. 250. $25.00.)

Nuclear Proliferation in the 1980s: Perspectives and Proposals. Edited by William H. Kincade and Christoph Bertram. (New York: St. Martin's Press, 1982. Pp. xiv + 272. $27.50.)

It is one of the ironies of history that the Reagan administration may well be remembered as the one that stimulated the antinuclear weapons movement more than any other American administration since World War II. By intensifying the cold war, by presenting a view of the world that sees the forces of good arrayed against the forces of evil, and by surrounding himself with advisors who have talked about fighting and winning a nuclear war, Ronald Reagan has succeeded in moving the nuclear issue to the top of the public agenda. This increased awareness is reflected in the substantial number of books appearing recently that address different dimensions of the nuclear issue. The two books reviewed here should be welcomed for keeping this issue before the public, but, unfortunately, they are not both of the same quality and usefulness.

The Barnaby and Thomas book, *The Nuclear Arms Race,* is a first-rate collection of papers given in September, 1981 at the annual meeting of the General Section of the British Association for the Advancement of Science. The contributors are a distinguished group of scientists, physicians, political scientists, and historians including Frank Barnaby, John Erickson, John Humphrey, George Rathjens, Joseph Rotblat, and E. P. Thompson. As with most books of collected conference papers, there are the usual problems of unevenness and repetition, but on the whole this book suffers less from those problems than most.

The opening selection by Frank Barnaby offers a comparison of American and Soviet nuclear capability based on data provided by the 1981 *SIPRI Yearbook.* New here is Barnaby's emphasis on the point that policy is changing from deterrence to war-fighting primarily as a result of technological innovations by military scientists. A minor point, but one wonders about his assertion that Pershing II, with a range of 1,500 kilometers or twice that of Pershing I, can reach Moscow from the Federal Republic of Germany (p. 28). David Owen's call for increased conventional armament for NATO permitting acceptance of a no-first-strike-agreement is unexceptional, but the chapter by E. P. Thompson is worth the price of the book. His observation that some American defense experts suffer from an infantile political view of the world derived probably "from too much early reading of Tolkein's *Lord of the Rings*" (p. 57) is summarized in the following sentence: "The evil kingdom of Mordor lies there, and there it ever will lie, while on our side lies the nice republic of Eriador, inhabited by confused liberal hobbits who are rescued from time to time by the genial white wizardry of Gandaf-like figures such as Henry Kissinger, Zbigniew, Brzezinski, or Richard Allen" (p. 57).

John Erickson's survey of the Soviet view of deterrence is especially useful as a rebuttal to those who have used several Soviet sources to suggest that the Russians are planning on fighting and winning a nuclear war. Pointing out the difference between statements of ideological rectitude and the much more cautious line found in professional military writing, Erickson suggests that "Soviet opinion seems to hold that nuclear war is not a rational instrument of policy, for means and ends lose any significance when the cost of destroying the enemy amounts to self annihilation" (p. 78). Egbert Boeker's call for the denuclearization of Europe and the conversion of NATO forces into a nonaggressive defense is worth considering as a way out of the seemingly endless confrontational posture of NATO and the Warsaw Pact.

Joseph Rotblat's discussion of the physical and medical effects of nuclear weapons is worth reading for someone who has not yet read Jonathan Schell's more detailed and chilling account; Sidney Butler's essay covers some of the same ground. George Rathjens points out that mutual assured destruction is not a doctrine, but rather a description of the physical reality of the contemporary world for the last 20 years. Stressing the uncertainty of the outcome once nuclear weapons are used, Rathjens questions the possibility of keeping a nuclear war limited either at the strategic counter force level or at the NATO battlefield level. Nicholas Sims is very cautious about the possibilities of diplomatic negotiations leading to nuclear weapons limitation or reduction, but he does not see what other alternative there is to the slow and tedious bargaining process. Nigel Young's concluding essay would have followed more logically after Thompson's, since both men are convinced that progress toward a more sane world will have to occur at a level below governments because political and military leaders on both sides are locked into a confrontational orientation that offers hope of nothing but continual arms racing or worse. Overall, this book of highly readable essays is worth the time of the general reader with an interest in the fate of this world.

The same cannot be said of *Nuclear Proliferation in the 1980s,* a collection of essays by younger scholars who won an international competition to participate in a conference on nuclear proliferation. The editors claim that one of the values of the book is that they have made a deliberate effort to include the views of people from the Third World. However, six of the 12 contributors are students in American schools, two teach at American universities, one works for an American consulting firm, one is a student in West Germany, and one is a student in Switzerland. Only one is a student in India.

The principal problem with the book is that all of the papers are dated. The sources cited indicate that the papers were written in 1978 or 1979, and yet no explanation is offered for why it took so long to bring these papers into print. In some fields being slightly out of date is not a serious problem; in the field of nuclear development, it is critical.

The papers are organized into four parts, the first of which attempts to explain why nations have gone nuclear. The second section considers the pressure to go nuclear experienced by the "garrison states" of Israel, South Africa, South Korea, Taiwan, and Pakistan, and the "prestige states" of Argentina and Brazil. In the third section that considers limitations on attempts to prevent proliferation, Cynthia Cannizzo's criticism of the technical approach is noteworthy. The fourth section, offering alternatives to the present Nuclear Nonproliferation Treaty, is primarily speculative and sometimes filled with observations such as: "A sense of responsibility will also be required from other nations" (p. 228). It is hard to tell after reading all of these diligent efforts precisely what one has learned. Perhaps because almost 10 years have gone by since the Indian explosion and no new country has given tangible evidence of having developed nuclear weapons, the issue of horizontal proliferation does not seem anywhere near as important as the vertical proliferation practiced and planned by the two superpowers.

PHILIP W. DYER

University of Nebraska-Lincoln

Mimicking Sisyphys: America's Countervailing Nuclear Strategy. By Louis Rene Beres. (Lexington, Mass.: D.C. Heath, 1983. Pp. xiii + 142. $19.95, cloth; $7.95, paper.)

Mimicking Sisyphus: America's Countervailing Nuclear Strategy is an articulate, polemical analysis of how this country's nuclear war-fighting strategy undermines our national security. Suitable for undergraduates as well as graduate students, the book helps fill the vacuum of accessible books in the arcane realm of strategic theory.

In this concise volume, Beres reviews the fundamental tenets of U.S. nuclear doctrine, devoting a chapter to each of five core assumptions:

> The Soviet Union might decide to launch a *limited* first strike against the U.S. and its allies;
>
> the Soviets are more likely to be deterred by limited U.S. counterforce reprisals than by total retaliation;
>
> the United States can survive a nuclear war with the Soviet Union;
>
> a major arms buildup can be maintained without compromising the prospects for arms control;
>
> peace can be maintained indefinitely through nuclear deterrence.

Beres is especially persuasive in examining the first two assumptions. Analyzing the United States' declared intention to "prevail" during a nuclear conflict, Beres captures the countervailing strategy's central flaw: "The Soviet Union is no more likely to be deterred by an adversary that has announced its intention to dominate escalation processes during a nuclear war than by one that remains content with the capacity for assured destruction" (p. 23). In fact, Beres argues cogently, an American counterforce capability would be perceived by Moscow as a declaration of U.S. intent to launch a first strike, thus creating incentives for any rational adversary to launch its own missiles in a preemptive strike.

Throughout the book, Beres does well to emphasize the overriding importance of how the Soviets view American nuclear strategy. However, in his readiness to assess Soviet perceptions, Beres tends to accept at face value Soviet rhetoric regarding their own nuclear doctrine, neglecting to question sufficiently the professed Soviet strategy of all-out nuclear retaliation. Moscow is also pursuing a counterforce capability in developing its new MIRVed ICBM, which means the Soviets' no-first-use pledge must be viewed with some skepticism.

Looking into the future, Beres presents persuasive arguments for abandoning the current doctrine of flexible/incremental nuclear response in favor of a more stabilizing policy of minimum deterrence. His near-term agenda for improving U.S. national security is both desirable and politically realistic: a comprehensive test ban, a more extensive U.S. nonproliferation commitment, and a higher nuclear threshold in Europe (achieved through a withdrawal of battlefield and theater nuclear forces coupled with a strengthening of NATO conventional forces). Beres's analysis is weakened somewhat, however, by an overly detailed philosophical discussion of the imperative of creating a new world order.

Overall, *Mimicking Sisyphys* is a timely critique of American national security policy, notable for its accessibility and provocativeness. In unpacking the hidden assumptions behind such misguided operational concepts as "limited nuclear war" and "escalatory ladders," Beres forces students of U.S. military doctrine to examine critically their own paradigms of national security.

ANNE H. CAHN

Committee for National Security

The Modern International Law of Outer Space. By Carl Q. Christol. (New York: Pergamon Press, 1982. Pp. xiii + 932. $85.00.)

Studies on emerging substantive areas of international law, such as communications, interstate pollution, and outer space, usually fall into one of three categories: historical surveys that trace the evolution of legal policies, reports and analyses of current developments, and detailed treatises that attempt to distill legal principles and policies. Christol's sprawling book embraces all three categories, with varying emphasis in each.

The book is organized along the lines commonly found in studies of the first category, historical surveys that trace the evolution of legal policies. Introductory and concluding chapters are separated by individual chapters devoted to a series of substantive topics. Christol concentrates his efforts on these topics by either analyzing the evolution of a particular convention, specific article, declaratory instrument, or issue area. The range of topics includes the 1967 outer space treaty, the 1972 liabilities caused by space objects convention, protection of space from contamination, the 1968 rescue and return of astronauts agreement, the 1979 moon treaty, exploitation of national resources, delimination of outer space and use of the geostationary orbit/spectrum, direct television broadcasting, remote sensing, use of nuclear power sources, and the implications of the space shuttle.

Christol has succeeded in assembling a primarily historical survey that is a detailed account of the legal and political debates and is derived from primary documentary sources. Each chapter is formidably footnoted with excellent references to the appropriate preparatory works and conventions with secondary source material included. Without a bibliography, one must search through the notes for this material, however.

The result is an analysis of evolutionary developments in outer space law as seen through successive exercises in multilateral conference diplomacy. Successive proposals and compromises by the various participants are evaluated, intertwining both claims and decisions. Christol does not employ a grand legal framework of inquiry to order the vast array of facts within each major topic. Instead, he organizes the material around events and outcomes, a framework not necessary to the achievement of reasonably good results in his historical surveys, especially since outer space law has little in the way of custom or judicial decision to place in the context of a comprehensive legal approach.

The Modern International Law of Outer Space is also an excellent reference source for current developments in space and space communications law. Even though the book carries a 1982 copyright, events up to and including the UNISPACE-82 conference are described and evaluated. There is no better one-volume source for this material, including a valuable appendix that reprints the texts of the major agreements with their status of ratification. Thus, Christol succeeds admirably in the category of reports and analyses of current developments.

It is in the third category, detailed treatises that attempt to distill legal principles and policies, that more work needs to be done. However, it is clear that Christol does not intend this volume as an authoritative legal analysis. His view is that international law must identify values and perspectives if it is asked to govern activities in outer space, and that an understanding of substantive principles is advanced by detailing the negotiating history of the major agreements. Evaluated from his own premises, Christol has fulfilled his promise in what will surely be an enduring contribution to the field. What is lacking, and what could serve as a second volume to the present study, is a detailed interpretation of this negotiating history and a thorough search for community policies.

For example, Christol is cognizant that varied interpretations of principles such as "equity" and the "common heritage of mankind" have been the source of much misunderstanding and conflict, but consideration of such principles is scattered throughout his book. It would be valuable for a companion volume to emphasize the central principles that divide East and West, North and South.

I would recommend that any institution of higher learning with a program in international relations, law, or organization possess a copy of *The Modern International Law of Outer Space.* Certainly individual students of international law and policy would find it valuable, although the price will probably limit its utility as a text for a course, as will its intimidating length and somewhat dry style. However, policymakers and serious students of space, communications, and international law should not overlook this volume.

Eric J. Novotny

Communications Satellite Corporation

United States International Economic Policy in Action: Diversity of Decision Making. By Stephen D. Cohen and Ronald I. Meltzer. (New York: Praeger Publishers, 1982. Pp. xii + 214. $28.95, cloth; $12.95, paper.)

Cohen and Meltzer's purpose is to show how the process of international economic policymaking works in the U.S. by discussing the issues of trade, financial, and industrial policies. They report analyses of five recent decisions, which led respectively to tariff preferences for developing countries in 1975, a 1977 program to boost the shoe industry, measures to halt the dollar's decline in 1978, an unfulfilled promise of modest debt relief for poor countries, and the 1981 restraints on Japanese auto exports.

One of the book's two central arguments is that "U.S. government decisions in this area are reached through an accommodation of competing economic and political perspectives within the context of diverse policy issues, changing circumstances, and unexpected exigencies" (p. ix). The five episodes picture a pluralist polity normally producing internal compromises. For example, strong interests lobbied against as well as for import protection for shoe and car manufacturers.

The work's originality springs from Cohen's and Meltzer's interviews with Washington officials. Their accounts give an interesting an informative record of the political events in each episode and a strong flavor of what U.S. bureaucrats and others were thinking at the time, while also filling in needed background. To some extent the case studies also open windows for viewing the American policymaking process in general.

A substantial weakness is that Cohen and Meltzer do not generally provide comprehensive economic data on trends in the respective markets, making it difficult to see the full picture that policymakers were facing. Also, they shun microeconomic analysis, or generally the use of economic theory as a tool for explaining policy.

Unfortunately, the book's original contributions beyond this are thin. Cohen and Meltzer explicitly disclaim any effort to improve theories for explaining policy. Their second central argument is that the inherent complexity and imprecision of policymaking in this area preclude establishing a single, universally applicable model to explain it; "all available models are appropriate in specific circumstances" (p. 8).

The case studies are actually not organized to support the latter or any other general thesis. They do leave an impression of enormous complexity, but then there is never a sustained effort to spell out and use any particular simplifying model. The closest approximation is a superficial eight-page section that covers no fewer than 17 different concepts, merely asserting that all are useful but all are inadequate. None is stated precisely enough to allow it to fail empirically.

Cohen and Meltzer have chosen a familiar conception and method that maximize the difficulty of developing valid theories about this subject. They focus on the policymaking process, rather than on policy content itself, as the primary phenomenon to be analyzed. Very often this path leads off into a thicket of unique domestic details that prevents the analyst from seeing the forest, as has nearly happened here. And their method evidently did not include careful attention to research design. The cases were chosen "basically at random" (p. 8).

The value of the case studies is also reduced by analytical narrowness. They largely ignore lasting international and domestic structures that other scholars see as crucial influences on state policies toward industry, finance, and trade. This book sets aside "broad forces and historical developments," with the odd implication that only radicals write about such things (p. 193). The text is heavy with U.S. officials' own reasoning, but rarely asks what structural elements or shifts could have narrowed the options even considered.

All this leads Cohen and Meltzer into an excessively voluntaristic interpretation, exaggerating the range of likely outcomes and the difficulty of predicting or explaining. Other students have confirmed generalizations such as the hypothesis that the chances of protection are strongly associated with the size of the industry requesting it. Still other recent works have been making analytical progress by integrating the global level with the national. These alternative approaches take policy content itself to be the key question.

The case studies can be used profitably if allowance is made for these omissions. But progress in theory development will have to be sought elsewhere.

JOHN S. ODELL

University of Southern California

The Doctrines of American Foreign Policy: Their Meaning, Role and Future. By Cecil V. Crabb, Jr. (Baton Rouge: Louisiana State University Press, 1982. Pp. xii + 446. $35.00, cloth; $14.95, paper.)

Cecil V. Crabb, Jr. has singled out what he considers to be a unique characteristic of American diplomacy: reliance on doctrines to guide foreign affairs. Crabb views this tendency as somewhat peculiar in that American domestic politics is relatively pragmatic and nondoctrinaire. Eight principle elaborations of principles are covered: Monroe; the Open Door and Stimpson; Truman; Eisenhower, Johnson, first (Vietnam) and second (Dominican Republic); Nixon; and Carter. Each is dissected according to its genesis, the impact of public opinion, influences of executive agencies and Congress, and the application of the doctrine in world politics including the reactions of other nations, especially those upon whom the doctrines impact. Crabb concludes with a lengthy treatment of the role of doctrines in American foreign relations.

While the analysis of each doctrine is thorough, the overall effect is uneven. The journey through American diplomatic history using the doctrines as a focus creates such disconcerting chronological leaps as, for example, the jumps from the Truman Doctrine in 1947 to the Eisenhower Doctrine in 1957 to the first Johnson Doctrine (the Tonkin Gulf Resolution) in 1964.

Since some doctrines do not outlive the crisis that provokes them, such a treatment is appropriate. However, in the case of the Truman Doctrine—a vision of the international system and the U.S. role within it that has structured much of world politics since the end of World War II—such a treatment tends to give it equal weight with, say, the Stimpson Doctrine. The specialist would also find that limiting the discussion the program of aid to Greece and Turkey does not convey the full impact of NSC-68 and the Korean War of June, 1950 in giving the Truman Doctrine its comprehensive and compelling impact on U.S.-USSR relations, alliances, and the present-day arms race and U.S. relationships with the developing nations.

Crabb's views toward the various doctrines slides from praise to criticism within a mainstream interpretation of American foreign policy. He is certainly no cold war revisionist and occasionally makes an effort to specifically refute such interpretations. The doctrines under consideration vary in their content, their specificity, and their impact. Crabb makes clear that while the label doctrine implies uniformity, the doctrines under consideration range from wide-ranging visions of international order, such as the Nixon Doctrine, to rationales for specific policies, such as the Tonkin Gulf Resolution and Johnson's escalation of the war in Vietnam.

The most valuable part of the book is the conclusion, in which Crabb relates the issuance of doctrines to traditional themes in American diplomacy. The American perception of itself as "the city on a hill," untarnished by the tawdry power politics of the Old World, finds concrete form in these doctrines. The American propensity to issue unilateral declarations of enduring principles reflecting the unique role of the U.S., its belief in self government, its millennial mission, is contrasted with the actual origins of presidential doctrines that are often idiosyncratic, hurried overreactions to specific crises. Often such pronouncements are expected to have an effect merely because they are stated and reflect a lack of convergence between eternal principles and specific national interests and between those interests and the economic, political, or military means to achieve them.

Crabb sees no end to the practice of issuing doctrines, in part due to the American approach to foreign affairs, in part because they have served the nation well in some respects, and in part because presidents seem to like the word doctrine associated with their names.

The book could have been edited more tightly to avoid repetition, and Crabb tends to depend on standard secondary sources for his material. These are carping criticisms, however. The book, especially its conclusion, provides rich fare for those seeking to explore the American propensity to clothe the pursuit of national interests in the garb of enduring doctrines.

RICHARD B. FINNEGAN

Stonehill College

Israeli Nuclear Deterrence: A Strategy for the 1980s. By Shai Feldman. (New York: Columbia University Press, 1982. Pp. xiii + 310. $25.00, cloth; $9.95, paper.)

Rejecting Israel's policy of calculated ambiguity governing its nuclear bomb status, this study argues that Israel's deterrent capacity would be served by the country's open adoption of strategic nuclear deterrence based on first and second strike capabilities. Avoiding any admission that Israel has had nuclear bombs since the late 1960s, Feldman, an Israeli, contends that if Israel does not already have nuclear arms, it should develop them and introduce them overtly later on. According to Feldman, "nuclear weapons would provide Israel for the first time with the ability to threaten unacceptable punishment in retaliation to an Arab challenge to Israel's survival" (p. 54). How such a challenge could be mounted in view of Israel's military hegemony is not clear. Frightening Arab leaders is an essential purpose of disclosure. Countervalue city targets are discussed, but not the possibility of radioactive dust returning to Israel.

Important preconditions are attached to open Israeli proliferation. First, Israel should persuade Moscow that Israel's atomic bombs would not threaten the USSR. It is not clear why the Kremlin might fear 30 to 40 Israeli nuclear bombs. Second, "Israel should attempt to postpone the adoption of overt nuclear deterrence until a few more states have gone nuclear" (p. 242). Feldman believes that it would be desirable for an Arab or Moslem state to take the public step (and the onus) ahead of Israel. The book overestimates Arab nuclear bomb potentialities. Israel might have to wait a long time for another regional state to proliferate. Feldman holds that if adversaries gain a conventional edge, Israel should go public first.

Third, Israel's public adoption of a nuclear-bomb strategy must be accompanied by a "flexible political posture" (p. 243) towards Arab lands taken in 1967. Nothing is said about Israeli settlements promoted by Menachem Begin, who is mentioned only in a footnote. If a Palestinian state acquired nuclear arms, deterrence would keep it responsible. Feldman offers interesting evidence deflating perceptions of Arab leaders as irrational.

Feldman is probably right in estimating that the U.S. would not penalize Israel if its nuclear arsenal surfaced. Clearly, the U.S. has not exploited opportunities to try to denuclearize Israel. Postdisclosure, the U.S. would be apt to provide increasing amounts of aid in response to interest group and congressional efforts to relieve Israel's budget and to associate Israel's nuclear forces with U.S. Mideast defense. Such measures would erode U.S. nonproliferation policy world-wide.

The book's thesis is indebted to a Mideast balance of terror argument made by Steven Rosen in the *American Political Science Review* (1977, *71,* 1367-1383). Rosen is credited, but not Moshe Dayan, who recommended an Israeli bomb-on-the-table linked to Palestinian land concessions to safeguard Israel's security and its Jewish character. Diverging from Dayan's ideas respecting Palestinian self-determination and Arab nuclear bombs, Feldman's thesis conflicts with Likud and Labor satisfaction with the basement-bomb policy and the 1981 Iraqi raid, which presumably protected Israel's regional nuclear bomb monopoly. The Iraqi reactor assault is noted. No mention is made of Begin's preventive strike doctrine aimed at any adversary gaining even nuclear bomb-making capability, let alone possession of nuclear arms.

Intended to stir Israeli nuclear discussions, the book faces obstacles in a country where the topic of Israel's nuclear weapons is virtually never discussed in public. With notable exceptions such as George H. Quester, U.S. antiproliferationists have glossed over Israel and have focused on worrisome states like Argentina, the spectre of an Islamic bomb having a current potential only in Pakistan and Reagan relaxations of U.S. nuclear export practices. Whatever judgments might be made of Feldman's policy-oriented analysis, this study may draw U.S. attention to Israel's unacknowledged nuclear bomb status. The condition overshadows free-zone and other nonproliferation issues of the Mideast and enters into strategic nuclear calculations involving the region.

PAUL F. POWER

University of Cincinnati

The Crisis in Western Security. Edited by Lawrence S. Hagen. (New York: St. Martin's Press, 1982. Pp. 246. $27.50.)

The essays in this volume stem from an April, 1980 conference at the London School of Economics. They were revised by the authors before publication and reflect the state of nuclear concerns at the time of the very early Reagan administration. Even if the tone of the debate has switched to problems with armament from problems with arms control, the essays have not lost their usefulness. The central theme of the book is that arms control has been divorced from the military and political developments that give it meaning and that are necessary for its success. Although the analyses are diverse, the book is, in many ways, an autopsy of arms control.

The first two essays, Lawrence Hagen's and Philip Windsor's, give an overview of the failings of détente and arms control. While reflective, they point out a number of inconsistencies in détente and the SALT process. The following two chapters are more forward looking with respect to the prospects for arms control. Lawrence Freedman, in drawing out the lessons of the 1970s, suggests that "talking shops" replace the ongoing negotiations for agreements. This would more easily permit political conditions to lead to viable, although more limited, military agreements. Richard Burt also reviews the failure of arms control; what otherwise might be viewed as a redundancy in this volume might better be seen as a consensus. Burt's analysis leads to the recommendation that unilateral force improvements offer greater possibilities for stability. Arms control can only contribute a certain degree of predictability.

Colin Gray takes this a bit further and asserts that arms control may have a net negative impact on Western security. Defense policy or strategy should have a primacy over arms control efforts, which are just as likely to destabilize as control the right weapons.

Although a bit out of the arms control focus of this volume, Desmond Ball presents a short review of recent technological developments and their relation to changing nuclear strategies. Technological change has led to counterforce possibilities, and these have been incorporated into targeting doctrines. However, complex counterforce and controlled-response operations remain unfeasible.

Three essays deal with theater nuclear weapons in Europe (now INF). Lothar Ruehl provides a concise statement of the need for force modernization. Paul Buteux places INF in a more general light and questions whether the planned modernization will solve the deterrence problems associated with flexible response. Robin Ranger's prescient analysis details the near impossibility of progress in either strategic or European arms control.

The three concluding pieces examine the shifting underpinnings of arms control. In a balanced analysis, Pierre Hassner points out the differing U.S. and European perspectives and their implications for alliance cohesion and arms control. His notion of "comparative vulnerability" is a useful insight, if a bit underdeveloped here. Helmut Sonnenfeldt discusses the elements of East-West relations that are critical to arms control. Finally, Hugh Macdonald places the state of arms control in the context of changing attitudes towards the use of force.

An integrating essay would have improved this volume. All contributors agree that stability is the central objective of arms control and that previous efforts have not protected it. All seem to believe that a principle of parity is necessary to produce agreements, but that this is impractical and that it tends to erode the agreements once signed. These two concepts play large roles in the analyses, and differing views of stability and parity lead to quite different assessments of arms control. Interestingly, deterrence is mentioned only in passing and seems to have been replaced by stability. A major task remains to tidy these ambiguous concepts.

One wonders what a similar conference would produce today. Domestic politics and alliance consensus would no doubt figure more prominently. Perhaps the preoccupation with stability would be replaced by Michael Howard's notion of reassurance.

WILLIAM K. DOMKE

University of California, Davis

The Soviet Union and the Arms Race. By David Holloway. (New Haven, Conn.: Yale University Press, 1983. Pp. x + 211. $14.95.)

The nuclear arms race between the United States and the Soviet Union has, since World War II, been based theoretically upon a consociation of American superiority. Since the days of the Cuban missile crisis the Soviet Union—via an impressive incorporation of quantitative and qualitative increases in nuclear capabilities—has, according to Western estimates, moved from strategic nuclear inferiority, to parity in the 1970s, to superiority in the early 1980s.

According to David Holloway, a leading authority on Soviet defense policy, there are three elements of Soviet military policy that have heightened concern in the West. The first, often referred to as the Soviet military buildup, involves the "steady accumulation of military power over the last twenty years or so." The second is concerned with the nature of Soviet thinking about nuclear war and, most importantly, "its stress on the need to prepare to fight and win such a war." The third, often called Soviet expansionism, deals with "the increasing use of military power to expand Soviet influence throughout the world" (p. 1).

Yet, as is often asked in the West, what is it that drives Soviet military policy? In helping to answer this question Holloway examines three aspects of Soviet military power: the role of the military in the formulation of the Soviet state, Soviet views about the uses of military power in the nuclear age, and the role of economic and political institutions in complementing the Soviet military effort. By concentrating on the historical experiences, policy objectives, and institutions that "sustain" the Soviet military commitment, Holloway, unlike many of his contemporaries who explore esoteric paradigms and worst-case analyses, shows persuasively that Soviet military policy is a "product of specific decisions taken in a distinctive institutional setting, under the influence of a particular historical experience" (p. 178).

Holloway artfully handles the often-asked question of whether there is a Soviet military-industrial complex—by noting that the mere existence of a large defense industry and powerful armed forces does not mean that they can dictate Soviet defense policy (p. 159). Likewise, within the system of economic planning and management, in many ways a parallel system of military and civilian interaction and cooperation, "the priority of defense has been built into the economic system" (p. 176).

The Soviet leadership, comprehending the utility of treating nuclear weapons as instruments of military power and political influence, "has built up powerful nuclear forces not only in preparation for a possible nuclear war, but also to deprive the United States of any political advantage that superiority might bring" (p. 179). According to Holloway:

> Perhaps the most important conclusion is that the Soviet Union does not have satisfactory answers to the problems posed by nuclear war and nuclear weapons. The answers the Soviet leaders have arrived at are not very different from those given by Western governments: nuclear weapons are both instruments of power and influence, and the potential agents of catastrophic devastation. Nor are Soviet answers much better than Western answers. The Soviet Union has not been able to escape from the threat of nuclear annihilation. Its leaders and its people share our predicament. We have a common interest in preventing nuclear war. (p. 182)

Lastly, Holloway recognizes clearly that arms control, in many ways the cornerstone of the East-West political and strategic relationship, will be unlikely to lead to anything better than a regulated arms race (p. 180). Nonetheless, Holloway's prescription is sound: Negotiations to halt the nuclear arms race should be pursued as vigorously and seriously as possible. As for an American attitude towards the post-Brezhnev leadership, it seems, "at the very least, extremely ill-advised for the United States to pursue a policy that makes any Soviet move towards accommodation and cooperation less likely" (p. 182).

In conclusion, David Holloway's analysis of the Soviet Union's role within the nuclear arms race is, without question, a definite contribution to the growing body of Western literature dealing with the Soviet-American nuclear relationship. Objectively written and well documented, this work should appeal to the scholar and student interested in strategic and security studies, arms control, Soviet and East European studies, and Soviet-American relations in the nuclear age.

JOHN M. CARFORA

University of New Haven

Biopolitics, Political Psychology and International Politics: Towards a New Discipline. Edited by Gerald W. Hopple. (New York: St. Martin's Press, 1982. Pp. xiii + 195. $22.50.)

In reading through the diverse selections of this book, one is reminded of Tocqueville's observation about the different ways in which historians in aristocracies and democracies attempt to explain events in the world. The former tend to attribute causation to one or a few great men, while the latter, seeing no truly great individuals in their midst, seek to place causation in general forces beyond the control of mere men. In ways befitting Tocquevillian democrats, the authors of this book present their fledgling attempts to uncover certain nonrational "internal determinants" that would explain the decisions and actions of leaders in the area of international politics. The authors do not openly address the possibility of "great men" (they would never use such a phrase) acting in rational ways to influence events; rather, they are concerned with the forces that might influence the reason of these and lesser beings.

Both the boldness of the enterprise, and the authors' own uncertainty about it, are captured in the book's subtitle (which, curiously, is found only on the book-jacket and not on the title page): "Towards a New Discipline." The etymology of "discipline" nicely embraces the prospect of imparting useful advice to disciples (and rulers), while "towards" conveys the appropriate hesitancy attending the current embryonic state of their search for the controlling "forces." To the authors' credit, they are not at all loathe to confess the limitations of their respective fields of study. As Hopple notes, they "have no comprehensive, nomothetic theories of political psychology and international politics, our methodologies are fallible and in need of refinement, and progress on the policy relevance front is quite clearly limited" (p. ix). In fact, he ultimately concedes that "we can point to the relative—but not absolute—irrelevance of biopolitics and political psychology in the study of international politics" (p. xii).

The true reasons for this irrelevance—and the explanation as to why neither biology nor psychology are ever likely to have much importance for the practice of international politics—go beyond fundamental questions of whether "internal determinants" actually exist and, if so, how influential they really are. In point of fact, information about leaders' "cognitive processes," "memory-related factors," "values," "beliefs," and "biological processes" is available, if at all, almost exclusively in democracies. Since even democratic leaders are unlikely to submit knowingly to the requisite physiological examinations, researchers are restricted to such things as informants' (possibly biased) revelations and content analyses of speeches. But since even in democracies leaders' speeches are routinely written by professional speech writers, how reliable are psychological assessments of their public utterances? When we then turn to leaders in nondemocratic regimes (the category into which the majority of nations fall), one who wishes to analyze leaders from a discreet (and safe) distance is confronted by awesome difficulties. Hopple briefly discusses this form of "remote assessment," but his mention of largely unspecified "innovative techniques" lacks credibility.

To judge by the authors' almost unanimous expressions of hope and commitment, they are undeterred by their singular and collective lack of success to date in locating reliable indicators of "internal determinants." Whatever limited promise some of their research may have for other areas of human activity, in international politics it is most unlikely that they will ever be able to provide much of the practical guidance they boldly set out after in this book.

WILLIAM D. RICHARDSON

Georgia State University

Soviet Policy and Practice toward Third-World Conflicts. By Stephen T. Hosmer and Thomas W. Wolfe. (Lexington, Mass.: D.C. Heath, 1983. Pp. xviii + 318. $31.95.)

The Soviet Union's intervention in Afghanistan in 1979 together with Soviet-Cuban intervention in the Horn of Africa and Angola have renewed attention on Soviet policy toward the Third World as well as on her power projection capabilities to intervene in conflicts arising there. While not the pioneering work of this renewed attention, *Soviet Policy and Practice toward*

Third-World Conflicts is a significant contribution to it.

Hosmer and Wolfe have divided their book into two parts. Part 1 is a historical survey. In this part the authors trace Soviet third-world policy to the Stalin era and the evolution and expansion of Soviet aid, both economic and military, to the Third World; discuss the shifts in priority among the various regions of the Third World; and survey the third-world conflicts in which the Soviet Union has intervened or threatened to intervene militarily.

The more analytical Part 2 begins with a summary of what Hosmer and Wolfe see as the main Soviet third-world policy objectives followed by a discussion of patterns of pre-Afghanistan third-world Soviet military involvement. Logically, Hosmer and Wolfe end with projections concerning future Soviet military intervention in the Third World and the implications that these have for future U.S. policies and practices.

This book has many clear strengths but also a few weaknesses. Hosmer and Wolfe present an excellent discussion of the evolution of Soviet military involvement policy in the Third World. The discussion is placed carefully in the context of doctrinal premises and justifications of Soviet theoreticians. Thus, changes in actual policy as pursued by Soviet leaders reflect corresponding shifts in doctrinal discourse and vice versa. A second strength is the excellent discussion of the evolution of Soviet military aid and involvement; appropriate and comprehensive data as well as instances accompany the discussion.

A third strength in the book lies in the analysis in Part 2, especially in the discussion of the patterns of third-world military involvements and projections concerning the future of such involvements. Two of Hosmer and Wolfe's arguments will surprise some readers. One is that "Soviet military involvements in the Third World . . . have come largely in response to indigenous developments not of Moscow's making or to the action or inaction of other outsiders" (p. 135). Many observers contend that the Soviet Union is actively seeking to expand its influence in order to create opportunities for themselves or "stir up trouble" in the fluid Third World.

The other surprising contention is that the Soviets have tended to intervene only in low-risk situations and have avoided the risk of a military confrontation with the U.S. Again, the cautious Soviet posture indicated in these two arguments would be disputed by those who hold that the Soviet Union is a warmonger with a proclivity toward adventurism.

A final strength of the book is the extensive 33-page bibliography. This will provide a convenient source of references for those interested in the subject matter of the book.

As noted, there are a few minor weaknesses in the book. First, it is "spread thin," thus denying the reader an in-depth analysis of some issues. Second, the analytical section, Part 2, is a bit short and consequently too brief in some instances. Third, in chapter 6, it is not entirely clear how the following relate to the subheading *Soviet Intervention in the Middle East and Africa:* (1) the two paragraphs on the construction of naval support facilities at Cienfuegos, Cuba (p. 48); (2) the paragraph on the two airlifts to Peru and Bangladesh (p. 48); and (3) the paragraph on the second Indo-Pakistani war (pp. 49-50). Finally, in the absence of a discussion of the structure of Soviet power-projection assets or of defense systems in general, the following terms, which are not in the glossary, might be distracting to the uninitiated reader: Antonov transports, LSMs, LST, Kyanda-class missile cruisers, Alligator-class LSTs, and Def Con 3 alert.

In conclusion, although somewhat spread-thin, this book by two Rand Corporation scholars is serious, lucid, and thoroughly documented. Hosmer and Wolfe are intimately familiar with third-world Soviet military involvement policy and practice, and their book will be useful reading to a wide and varied audience.

HARRY ODODA

Russell Sage College

The United Nations and the Control of International Violence: A Legal and Political Analysis. By John F. Murphy. (Totowa, N.J.: Allanheld, Osmun Publishers, 1983. Pp. xii + 212. $32.50.)

Murphy is a lawyer; he was an attorney with the Office of the Legal Adviser of the U.S. Department of State and is now an academic professor of law. Thus, in spite of some explicitly stated social science ambitions, his book is a lawyer's book. It is primarily a descriptive and evaluative analysis of the historical record and future prospects of United Nations' efforts to deal with acts or threats of international violence. As such, for the most part, it is modest, informative, reasonable, and fair.

For a book of the analytical intentions that this one has, however, moderation is not enough. Murphy tries to evaluate overall United Nations' experience in the peace and security area, based on the evidence of the U.N.'s entire record. Certainly, the whole range of that record is covered, from the presence of Soviet troops in Iran in 1946 to the Falklands/Malvinas conflict in 1982. But it is covered selectively. Although the selection is admitted freely, it is nowhere explained. No analysis is offered, and sometimes no mention made, of Greece, the Balkans, Indonesia, Hungary, Algeria, Bizerte, the Bay of Pigs, the Dominican Republic, Goa, India-Pakistan, Chile, and Afghanistan. Not addressed are the issues of why these cases were excluded and how, if at all, their possible inclusion would have affected the interpretive or prescriptive conclusions that this study reaches.

More importantly, Murphy divides the otherwise quite numerous and fairly representative cases that he does consider into two separately analyzed groups: the "traditional" and the "nontraditional" uses of international violence. Within the traditional group but not the nontraditional, he further subdivides his analysis of U.N. behaviors by institution of agency: The Security Council did these, the General Assembly did those, etc. It is never clear why. In any event, all of these divisions are almost pointless and often distorting. The Suez crisis of 1956 thus is considered as a problem addressed by the Security Council, although most of the work and all of the inventiveness of handling it are later correctly ascribed to the General Assembly and, especially, to the Secretary-General. The larger traditional/nontraditional distinction likewise comes to no useful end when it is admitted (p. 165) that all of Oscar Schacter's listed "ten ways in which the United Nations may play a role in . . . internal [i.e., 'nontraditional'] conflicts" have been employed by the U.N. "in a variety of other circumstances as well." One looks in vain for analytical rigor, at least from a social science perspective, anywhere in this study.

The United Nations and the Control of International Violence has its uses, however, and its pleasures. With the exception only of his confusing and sometimes inaccurate account of the Korean issue (the Uniting for Peace Resolution was never, as such, used in Korea), Murphy's descriptions of a great variety of U.N. activities in respect to sometimes quite complex issues are models of graceful concision. His earlier summaries of pre-U.N. and pre-League of Nations developments in international organization, although admittedly heavily dependent on Inis Claude, are informative and spare. His judgments, again, are modest and fair. Persons wanting an interesting, current, moderate, and almost always reliable summary of most of the U.N.'s most important peace and security efforts will find one here.

Students of international relations should read *The United Nations and the Control of International Violence.* Murphy's fellow legalists may want to do likewise. Social science research scholars will not need to.

KEITH S. PETERSEN

North Carolina State University

The International Law of the Sea, Vol. 1. By D. P. O'Connell. Edited by I. A. Shearer. (New York: Oxford University Press, 1982. Pp. xxxii + 634. $74.00.)

In December, 1982 delegates from 151 countries to the Third United Nations Conference on the Law of the Sea met in Montego Bay, Jamaica, to sign a treaty forged over nearly 15 years of negotiation. The 439 articles of the text reflected agreement on complex issues including ocean boundaries, fisheries, deep seabed mining, navigation and shipping, pollution control, and scientific research. Despite this formidable achievement in modern conference diplomacy, continued conflicts over ocean use and resources—highlighted by the United States refusal to sign the treaty—insure that a stable legal order for the oceans will remain an elusive goal for the international community.

The philosophical, intellectual, and juridical evolution of the doctrines and principles fundamental to this legal order are the subject of this monumental work by D. P. O'Connell. O'Connell, Chicele Professor of International Law at Oxford, had completed the chapters for this two-volume book before his death in 1979. I. A. Shearer, Professor of Law, University of New South Wales, assumed the task of preparing the text for publication and incorporating pertinent developments between 1978 and 1981.

In many respects, "the history of international law is the history of the law of the sea and vice versa, for the intellectual character of international law, its techniques, and its philosophy, have been largely determined by the accommodations reached among nations respecting the use of the sea" (p. 29). Much of this has been determined by pragmatic decisions, determinations of interest rather than philosophical concepts. Consequently, "it was the facts of power and the pragmatic needs of commerce and strategy that eventually endowed the rules of the law of the sea with whatever stability they came to possess" (p. 29). But the proliferation of states and unilateral maritime claims since the end of World War II make questionable the prospect of a stable rule of law discernible in state actions. In fact, state actions, such as the United States' unilateral proclamation of an exclusive economic zone, reflect a desire to establish new rules quickly rather than any commitment to old rules.

Competition between the exercise of governmental authority over the oceans and freedom of the seas has recurred for nearly 400 years. O'Connell argues persuasively that the historical method is the only one with the power to illuminate the origins of the rules and doctrines that have shaped the law of the sea, to describe the historical and political context that formed them, to determine how they have changed with experience, and to assess their intellectual integrity. This historical perspective provides a framework for O'Connell's comprehensive and detailed analysis of the major topics in ocean law: the juridical nature, extent, and measurement of the territorial sea; archipelagoes; innocent passage in the territorial sea; passage in international straits; internal waters (bays, ports, straits); the legal regime of the seabed; the continental shelf; fisheries zones; and the recently created exclusive economic zone. (Volume 2 will analyze jurisdictional issues affecting pollution, scientific research, shipping, navigation, and port access, as well as expand discussion of maritime boundaries, and the delimitation of the territorial sea, the continental shelf, and the exclusive economic zone.)

O'Connell traces each topic from its origins, its evolution in the treaties and opinions of jurists such as Grotius, Bynkershoek, Vattel, and Galiani; debates within the academic community; and through efforts at codification at the Institut De Droit International during the 1880s and 1890s; the Hague Codification Conference of 1930; the International Law Commission (1950 to 1956); and First (1958) and Second (1960) Geneva Conferences on the Law of the Sea; and the Third United Nations Law of the Sea Conference (1974 to 1982).

Throughout this detailed and extensively documented work, O'Connell provides an erudite and intricate exegesis of the doctrines and principles of the law of the sea. The book's intellectual contributions are further enriched by its extensive documentation, bibliography, list of national legal cases and international incidents, as well as indexes of topics, authors, and conventions. It is a work of prodigious scholarship destined to find a prominent place in the literature of international law.

LAURISTON R. KING

Texas A&M University

Arms Control and East-West Relations. By Philip Towle. (New York: St. Martin's Press, 1983. Pp. 187. $30.00.)

Believing that arms control has a constructive role to play "in the intermediate band of relationships" (p. 2), Philip Towle, Fellow of Queen's College, Cambridge, and Deputy Director for International Studies at the University of Cambridge, argues that hopes for arms control were too high in the 1960s and 1970s. He seeks to describe the conditions under which arms control may again play an important role in East-West relations.

In the first part of the book, Towle describes a number of different approaches to controlling war. The first of these, disarmament, has little appeal for him, for unlike "disarmers" who blame wars on armaments, he believes that war is caused by "politics, by the desire of revisionist states . . . to change the *status quo*" (p. 3). Towle illustrates most of his points with historical examples. Although he makes one caustic remark about quantitative studies of arms races (p. 15), there is little evidence in text or footnotes that he has read any of the relevant quantitative literature.

Towle next discusses arms control. Dealing with both pre- and post-World War II cases, Towle inexplicably makes no mention of the standard works on SALT-I and SALT-II. He reaches the pessimistic conclusion that "efforts to negotiate any important arms control agreements will only be effective if detente can be restored. . ." (p. 31).

The third approach Towle considers is the "political," by which he means attacking a problem more directly than the way arms control addresses the East-West conflict. He illustrates this by discussing the Conference on Security and Co-operation in Europe (CSCE) on the ground that this negotiation has sought "to tackle the causes of fear and suspicion" (p. 41).

A fourth approach, the "humanitarian," leads Towle to discuss the laws of war, while a fifth, the "pragmatic," moves him to illustrate techniques the superpowers have used to limit their involvement in third-world conflicts.

Following the chapters on the five approaches, Towle discusses the contrasting views on arms control and disarmament of the Soviet Union, the Western democracies, and the Third World. Concluding that the Soviet Union has good reason to avoid a nuclear war, Towle asserts that "within . . . limitations [the Soviets] are prepared to negotiate measures designed to reduce the possibility of war with NATO" (p. 97). After a final section on verification, Towle concludes his argument with a call for "a wide-ranging politico-military agreement with the Soviet Union." As Towle phrases it,

> If the arms control agreements included in such a wide-ranging rapport were properly verified, the whole package could help to reduce the swings between detente and cold war, which have characterized the post-1945 period and circumscribe ideological competition between the two blocs. In the foreseeable future that is the most we can hope for. (p. 178)

While this vision is one which many readers might share, the key question is whether the previous 177 pages are necessary to reach this destination. To this reviewer, the answer is negative.

Towle's book contains some interesting historical examples ranging over the nineteenth and twentieth centuries, and he draws extensively on primary documents from the United Nations and the British Foreign Office. In the final analysis, however, the argument does not hold together. It would, perhaps, have been better if the scope of the work were less ambitious. In that case Towle might have used his talents to write a long essay on a focused thesis rather than a short book that has insufficient central direction.

ROBERT C. GRAY

Franklin and Marshall College

Two Hungry Giants: The United States and Japan in the Quest for Oil and Ores. By Raymond Vernon. (Cambridge, Mass.: Harvard University Press, 1983. Pp. xi + 161. $16.00.)

The urge of industrialized nations to secure reliable sources of basic raw materials is hardly a new theme in modern international relations. The ensuing competition for these materials provides opportunities for both cooperation and conflict. While these clashes do not make for major political crises, they are the essential backdrop of bilateral relations between nations. *Two Hungry Giants* is the first book of its kind to explore systematically the relationship between Japan and the United States in terms of the competition for industrial raw materials under a variety of market conditions (glut and shortage) and each country's respective long-term and short-term policy responses.

A few figures indicate the importance of raw materials for Japan and the U.S. In 1979, Japan and the U.S. consumed 46% of the world's crude oil outside the Communist bloc, 53% of its iron ore, 36% of its copper, and 46% of its coal. Vernon chooses to focus his analysis on crude oil and oil products, iron ore, bauxite, and copper ore, fully cognizant of the risk that problems arising in other key commodities might be overlooked. He posits that generalizations can be drawn from these commodities to others like uranium, chromite, and manganese. This is not necessarily so.

Vernon's thesis is that the two countries could not have responded more differently to similar problems and that their responses to a similar problem (that is, securing raw materials sources) stem largely from each country's distinct history, culture, and geography. He posits that, at least in this field of policy, national policymakers are the "prisoners of their national environment," through war and peace, gluts and shortages. This type of broad "environmental" determinism is an explanatory construct that has been downplayed by the historical school in recent years, but which has a long tradition in international relations (see, for example, the work of Harold and Margaret Sprout).

The first chapter defines the broader U.S.-Japanese economic and security relations that are characterized respectively as "an endless string of capitulation (by Japan) under (U.S.) pressures" and the "passing of the ward-guardian relation." Chapters 2 and 3 examine, in turn, the world markets in oil and minerals and metals, concluding in both cases that the likelihood of continued market instability in the 1980s is fairly high. Chapters 4 and 5 consider respectively U.S. and Japanese principles and practices governing raw materials policy and detail how each is a prisoner of its history, institutions, and ideologies. Vernon shows clearly how Japan has learned to cope with its have-not status through flexible and inventive national policies implemented through close government-industry cooperation. Vernon considers that Japan has been remarkably successful in procuring its basic needs through a highly rational approach of careful diversification of sources, strategic stockpiling, and taking equity ownership overseas gradually. In Vernon's view, the U.S., on the other hand, lacks an explicit and consistent set of policies and has been divided consequently between protecting its domestic producers and securing reliable overseas suppliers. The upshot has been an unstable and wholly unreliable raw materials policy in which the U.S. has cast itself in "the role of the country bumpkin —unheeding before the fact, unprepared after it, and unable to learn from experience" (p. 124). The last chapter maps the political and economic imponderables within which future scenarios may be painted. Vernon concedes that forecasting and modeling of commodity markets in past years has been highly unreliable and, therefore, takes a less quantitative but more creative approach in considering the future.

Two Hungry Giants clearly defines the challenge for the two countries as the management of possible conflict over industrial materials in such a way that it does not disrupt the broader bilateral and regional security and trade relationships. Vernon concludes that while the postwar relations between the two countries on this subject have been relatively smooth when compared to auto trade, for example, the past is no guide to the future considering the looming supply, demand, price, and production uncertainties for the raw materials markets. Against this background, Vernon considers that "cooperation between the two countries on any sustained and significant scale is unlikely" (p. 123), but feels that the two are "doomed to continue as the odd couple" (p. 128).

Two Hungry Giants is an incisive, concise, and insightful treatment of an important international economic policy and problem. Its findings and approach have applications much beyond the specific policies and problems analyzed. This first-rate text has the additional virtue of combining an elegant use of both political and economic analysis. It should be useful to international relations scholars and nonscholars alike and essential to anyone focusing on international resource policy.

JOHN R. MCINTYRE

Georgia Institute of Technology

The Practical Negotiator. By I. William Zartman and Maureen R. Berman. (New Haven, Conn.: Yale University Press, 1982. Pp. xiii + 250. $20.00, cloth; $8.95, paper.)

In the words of the authors, this volume is an attempt to tell negotiators how to negotiate. And it is an attempt that succeeds brilliantly. But the book will appeal to scholars as well as to policymakers and negotiators. Zartman and Berman use three types of evidence—the historical record, "scientific" studies, and (of most interest to this reviewer) interviews with senior negotiators—to develop a model of successful negotiations that is at once intellectually compelling and useful as a practical guide for negotiators. The model identifies three stages in the process of negotiation—diagnostic, formula, and detail—and associates different problems and behaviors with each stage.

During the diagnostic phase, actors define the situation and decide to try negotiations. The possibility of a solution must be recognized by all parties for the negotiating process to begin. Negotiations are appropriate when the parties recognize the problem requires joint resolution, when they possess the political will to end a situation they consider unacceptable, and when they are willing to admit other parties' claims to participate in the solution. Appropriate behaviors during the prenegotiating period include: understanding the facts of the problem, examining precedents and referents, understanding the context and perceptions that define the situation, understanding the stakes and interests of all parties, becoming aware of the affective dimension, and beginning preliminary talks to persuade the other party about the negotiability of the problem.

The formula phase consists of reaching agreement on a formula or common definition of the conflict that facilitates solution. Zartman and Berman conclude that a deductive approach characterizes successful negotiations. Agreement on a framework for solution gives coherence and structure to subsequent agreements on details, helps facilitate the search for solutions on component items, and fosters a creative, positive image of the negotiations, rather than an image of concession and compromise. The authors recognize, however, that a deductive approach may not be possible in all situations. If not, they suggest the parties begin with an initial agreement on some details in order to create a positive atmosphere of progress that can lead to the emergence of a formula at a later stage. Appropriate behaviors during the formula phase include: maintaining a flexible mind set, focusing on the problem not the opponent as the "enemy" to be overcome, not being deterred by unfriendly behavior, adopting a "keep talking" posture, and thinking of detailed applications while considering the broad formula.

During the detail phase, parties negotiate the details required to implement the formula. It is in this phase that the art of concession-making plays a critical role. Zartman and Berman conclude that it is better to group, package, or exchange concessions rather than fight it out on separate issues considered individually. This "package" approach is consistent with the authors' underlying view of successful negotiations as creative rather than concession/compromise enterprises. Appropriate behaviors here include: not losing the big picture, being clear from the beginning about objectives and not confusing means with ends, and maintaining sufficient perspective on the negotiations to be willing to do without an agreement if agreement is not possible on your minimum terms.

The Practical Negotiator does not pretend to be a cookbook for aspiring negotiators. There is no one recipe for a successful negotiation. However, the experiences of successful negotiators do fit into a pattern, and negotiating skills can be taught. Zartman and Berman have made a significant contribution to the literature on negotiation. This book should be read by negotiators and scholars alike for valuable insights into the negotiating process.

LINDA P. BRADY

Annandale, Virginia

Normative Theory

Lectures on Kant's Political Philosophy. By Hannah Arendt. Edited by Ronald Beiner. (Chicago: University of Chicago Press, 1982. Pp. viii + 174. $15.00.)

Kant's Political Philosophy. By Patrick Riley. (Totowa, N.J.: Rowman and Littlefield, 1983. Pp. ix + 213. $28.95.)

These two studies of Kant are part of a remarkable revival of interest in the work of that philosopher whose thought has so deeply shaped what we mean by the modern outlook. Much of contemporary social science emerged from one or another school of "neo-Kantian" thought, although the connection is, unfortunately, rarely made an object of direct study today. Now, as Patrick Riley notes, John Rawls has done much to bring Kant back into currency (Riley calls the *Theory of Justice* a "neo-Kantian" work [p. ix]), although Rawls obviously also subjects Kant's thought to very serious modification (succinctly noted by Riley, p. 53 ff.; n. 104, p. 187). But the return to Kant is not exclusively shaped by Rawls; in a fine chapter, Riley examines recent works on Kant by Alexander Kojève, Lucien Goldmann, Robert Paul Wolff, Susan Shell, Hans Saner, and John Charvet, reminding us of a significant recent literature and developing his own position toward it.

Riley's own study of Kant is a major contribution to the discussion. With exemplary tenacity and open-mindedness, he examines the familiar thesis that Kant's approach to politics is one-sidedly formalistic. This view goes back to Kant's earliest critics and remains a staple. It is perhaps most deeply argued by Hegel, who made it a point of departure for his own effort to build a modern political philosophy adequate to the historical and the concrete (see pp. 38-42; 174). In our own framework, it is Rawls and Lewis White Beck who have recently defined the meaning of Kant's work. Riley argues that they have overemphasized the procedural formalism of Kant's teaching and, further, have presented him as a "deepened Rousseau" who teaches that law cannot be law unless a product of conscious will (pp. 53-58). This view makes human ends the product of will or construction, as if nothing can be an end unless some act of agreement establishes it.

It is the main goal of Riley's analysis to show that Kant advocates a critical teleology of objective ends, not of will legislating for itself. There are necessary ends for which we must act; this is ultimately a more important thesis for Kant than is the process of giving the law to oneself; the moral law is not constructed but somehow simply "there" (p. 56; cf. pp. 171-172). Riley must, of course, rely above all on Kant's late *Critique of Judgment* to support this thesis, but he possesses an extraordinary command of all of the texts and tracks down every source usable in defense of this uncommon reading of Kant's thought. Certainly he succeeds in showing that there is much to be found in Kant that will bear this interpretation. After this study, no one will be able to assert without further ado that Kant is either an unqualified formalist or a Rousseauian constructivist. Riley, however, admits that his pursuit of Kant's doctrine of objective ends establishes at best an "arguably" stronger reading than the usual one (p. 170), hence not a categorically stronger interpretation. He does not artificially minimize the formalist and constructionist strain, but would hold that he has succeeded in showing that Kant qualified or limited it.

A genuine dedication to the Kantian outlook underlies Riley's argument. The book gets its heart from this adherence. Noting a witty remark of Oakeshott that Kant is "important but not attractive" (n. 43, p. 206), Riley hopes to overcome this deprecation and eventually puts Kant very high. He is "*the* philosopher of freedom," who also knew how to see freedom in the light of the "moral law" considered as a "fact of reason"

(p. 5). In a passage at the end of the book, he steps out of his concentration on the texts and sources to suggest that Kant is "both the most important and the most attractive of political philosophers" (p. 176). Contrasting Kant with other moderns, he holds that Kant avoided the extremism to be found in Hobbes, Hegel, Machiavelli, Mill, Marx, Locke, Burke, Hume, Freud, and Nietzsche. Elsewhere, he does not deny a "Jacobin" strain and even a radical "humanism" in Kant's thought, however (pp. 132-133; 58-63). On the whole, the case for Kant's moderation would also need to be assessed through a thorough evaluation of the content he prescribes for the ostensibly objective ends (Riley's theme in the illuminating chapters 4-6).

Occasional flashes of humor in this book aid the argument for Kant's attractiveness by showing that moral earnestness is not totally fatal to wit.

Hannah Arendt's *Lectures on Kant's Political Philosophy* is a collection drawing together the *Postscriptum* to her *Life of the Mind,* a set of lectures on Kantian political philosophy given as early as 1964, then revised and presented on other occasions during the following decade, and some notes for a 1970 lecture on imagination. The compilation of these materials is quite useful, although readers will quickly discover that the lectures were never turned into finished compositions. Ronald Beiner, the editor, argues that they provide some important clues to the work on the faculty of judging that Arendt intended but did not live to write. The texts reflect many of the well-known mannerisms of her style. On the positive side, they also reflect her ability to cast an interesting, sometimes novel, light on famliar and unfamiliar issues. The whole collection does not add up to a complete account of the theme of judgment, or of Kant's political philosophy, as Beiner is the first to admit, but it does give some insight into the basis of Arendt's life-long interest in Kant, her interpretation of his political works and comments, and the faculty of judgment as she saw it after her extensive earlier reflections on thinking and willing in the *Life of the Mind.*

Like Riley's work, these studies of Kant focus on the *Critique of Judgment.* Arendt's interest is less on the doctrine of ends or *telos* than on the type of thinking that Kant calls judging and that is the way for "thinking the particular" (p. 119). This mode of thought, she holds, is that by which we combine the general and the particular to grasp politics. Ronald Beiner's extended interpretive essay, occupying about one-half of this volume, offers an illuminating study of Arendt's uncompleted work on judgment. Significant in its own right, the essay is perhaps most interesting in expounding and weighing some of the limitations in Arendt's work on this theme. An example would be the comments on the relative neglect of "prudence" in both Kant and Arendt. Beiner shows the need for a "confrontation of Aristotle with Kant" and directs us to work by Gadamer and others that explores this confrontation (p. 134 ff.). Ultimately Beiner seems to question not the importance but the sufficiency of both Kant and Arendt in their elucidation of this aspect of thought.

DONALD J. MALETZ

University of Oklahoma

Legitimacy of the Modern Age. By Hans Blumenberg. Translated by Robert Wallace. (Cambridge, Mass.: MIT Press, 1983. Pp. xxxi + 677. $35.50.)

Modern science buried centuries of theological controversy. Hans Blumenberg has unearthed these controversies again, rethinking the dilemmas and dead ends of Christian dogma that provided the intellectual provocations for the scientific revolution. Especially striking are his accounts of the deep strains between Biblical premises and Aristotelian and Stoic ideas within Scholasticism and of the emergence of voluntaristic nominalism, whose God was so thoroughly unfathomable as to be reasonably ignored. But Blumenberg has not merely written a scholarly, nuanced, and illuminating study of the religious background to modern science. He has also written a philosophical book, a combative response to the dim Romantic suggestion more common in Germany than America, that the modern age "as a whole" is somehow illegitimate. Tracing the many reasons why "the modern age is unthinkable without Christianity" (p. 30), Blumenberg sheds light on the legitimate claims of modern science and also reveals the historical poverty of all pre-staged duels between "the ancients" and "the moderns" that leave Christianity out of account.

Concerned to defend the Enlightenment against its enemies, Blumenberg is also aware that modern theorists have often defended indefensible claims. The fundamental error of the great modern philosophers, however, lay not in their wanton break with the past, but rather in their failure to make a sufficiently radical break. Christianity accustomed people to great questions such as, What is the purpose of human existence? and What is the meaning of history? Although Christian answers ceased to be credible, Christian questions lingered on, a disorienting legacy to the future. Rather than dismissing such questions by declaring them unanswerable in the positivist manner, modern theorists tried, unwisely but perhaps inevitably, to answer them. Thus, for instance, an initially modest idea of progress was overextended in a hearty but unfortunate attempt to answer the otiose question, What is the overall pattern of history?

Blumenberg fondly contrasts his concept of inherited questions with the widespread idea of secularization, for example, with Karl Lowith's notion that "progress" is a secularized version of Christian eschatology. Lowith's thesis is implausible, Blumenberg argues, if only because eschatology requires an external intervention for which people passively wait, while progress is an indigenous process driven by human effort. Indeed, far from being a secularization of the Christian idea of history, "progress" is a counteroffer, asserted against eschatology and its devaluation of the world. The needs of polemic may also explain some of the self-discrediting exaggerations of Enlightenment thought. A massive provocation will incite an exorbitant response. But when provocations recede from the cultural scene, as they have in the case of Christian dogma, historians must control their impulse to interpret, say, Hobbes's theoretical bravado as sheer willfulness or a sign of reprehensible hubris.

STEPHEN HOLMES

Harvard University

Secrets: On the Ethics of Concealment and Revelation. By Sissela Bok. (New York: Pantheon Books, 1982. Pp. xviii + 332. $16.95.)

Sissela Bok has ably complemented her analysis of lying (*Lying,* Pantheon Books, 1978) with further theoretical exploration of the moral and political underworld. This time it is secrecy that comes under Bok's scrutiny. As in her earlier work, the practice is treated in a wide range of personal, social, economic, and political contexts: personal secrecy, secret societies, confessions, trade and corporate secrecy, professional and scientific secrecy, secrets of state, whistleblowing and leaking, military secrecy, investigative journalism, intrusive social science research, and undercover police operations. Even gossip is discussed and, at last, defended (well, almost).

Bok's recognition of the wide scope of the practice of secrecy is complemented by the care with which she attempts to provide counsel in the face of this complexity. Neither secrecy nor total openness is advocated; disclosure of secrets may unleash the power of Pandora's box as well as topple the Sphinx. Further, secrecy both reaffirms a personal sense of self and separates us from others. Thus, unlike others who have inveighed against secrecy in a wholesale fashion, Bok offers a balanced view, recognizing secrecy's advantages as well as disadvantages.

Bok, however, attempts to do more than make a general case about secrecy; she aims to provide guidance about its numerous vexing dilemmas. Here the merits of the book become more mixed. In confronting practices of secrecy, we are advised to follow two moral "presumptions": (1) equality —whatever control over secrecy is legitimate for some should be legitimate for all, other things being equal; and (2) autonomy—partial individual control about personal matters. These are, however, only presumptions, and they still leave open the matter both of what secrecy is legitimate and what kind and degree of individual autonomy we wish to secure. They certainly are not principles, since they do not consistently structure or inform subsequent analysis. Bok resolves these omissions by appealing to particular cases and ascertaining what arguments for secrecy or disclosure might be made. For example, professional secrecy rests on three claims in addition to autonomy: the need to share with others, the weight of agreement between client and provider, and the general utility of professional confidentiality. Trade secrets rest on five claims: personal autonomy, property rights, incentives to invest, national security, and confidentiality. The merits of secret societies turn on their consequences for politics, other groups, the group itself, and its members. Bok then weighs the arguments of each of these claims.

It is not clear, however, that this approach can always provide acceptable moral or political counsel. Simply considering a range of arguments neglects to analyze what principles ought to be employed in ordering and discriminating among them. Bok's presumptions could perform this function, yet sometimes they are simply other arguments, not ways of discriminating among arguments. Even if we grant that they are the latter, problems arise. We should take seriously not only the need for principles but also how we derive them. What kinds of arguments we consider and how we weigh them may differ if we begin, not with a list of claims or arguments and then proceed to our intuitions about their merits, but rather first consider the needs and requisites of the particular institution, setting, or role, and deduce the principles they value. These mediating steps may both shade what kinds of arguments we discern and what principles we use in evaluating them. This approach is helpful where the practice we are examining occurs in a number of very different contexts, as with secrecy or lying. It seems especially important in those areas of secrecy of most interest to political scientists: secrets of state, leaking, whistleblowing, military secrecy, and law enforcement deception. Here political and institutional obligations and considerations ought to weigh more heavily, yet if we begin with our intuitions, we are likely to undervalue these constraints in favor of the personal and the individual.

To be fair to Bok, she does not wholly neglect institutions. She frequently acknowledges that structural, contextual, and organizational considerations influence the recognition of moral choice and the consequences we consider in making choices. This relationship of the normative and empirical is an important one and ought to be of particular interest to those who seek to separate political philosophy and political science. Moreover, constraints of role, institution, and setting do sometimes emerge in the arguments she considers and in the balancing of their merits. Their place in normative evaluation and institutional reform, however, needs to be both more centrally addressed and given proper weight.

JOHN P. BURKE

Williams College

The Concept of Class: An Historical Introduction. By Peter Calvert. (London: Hutchinson Publishing Group, 1982. Pp. 254. $18.24, cloth; $8.36, paper.)

Calvert has a clear thesis: that the concept of class is now muddled in theory, useless in scholarly application, and dangerous in politics. His work follows in the mold of a series on key concepts in political science for which he served as executive editor and as a contributor (*Revolution* [Praeger Publishers]).

Francois Quesnay made class an important economic term in 1759 when he distinguished between the "productive expenditure class" and the "sterile expenditure class," a distinction that became political particularly as the English associated class with rank (p. 25). On the other hand, Plato and Aristotle and their Christian interpreters had used the concept to investigate "class as balance" (chap. 2) between groups divided by ability, function, and natural attributes, an enterprise which led to consideration of class conflict. It remained for Marx to make of class the agent of history.

Calvert argues that Marx's work on class was incomplete, inconsistent in definition (p. 69), and contradictory (p. 83). Empirically, Marx confused class with estate (p. 100) and was wrong about capitalism's tendency to reduce all classes into the bourgeoisie and proletariat (pp. 135-136). Neo-Marxists are dismissed as whittlers who, in their attempt to sharpen bad wood, leave nothing of the stick (chap. 5). Capitalism, whether "advanced" or "late," has outlasted the rationalizations of Marx's position on class (pp. 142-143). Attempts by theorists such as Max Weber, Talcott Parsons, Seymour Martin Lipset, and Rinehard Bendix to give it more systematic meaning have foundered on the problems of objectivity/subjectivity and confusion of class with status, stratification, party, power, and relative deprivation. Operationalization becomes impossible where it isn't arbitrary and subjective as in Centers's upper-upper, lower-upper, etc., formulation (p. 114).

In the second, less scholarly half of the book, Calvert points to the homicidal uses of class in the hands of Mao, Stalin, and the Pol Pot regime. In analyzing Western democracies, Calvert concludes that what divides Britain "is not, in fact, a class structure at all" (p. 183), that class ill fits the American experience and, in fact, wasn't really introduced until the Great Depression, and that the 1981 election in France only demonstrates the achievement of a sense of continuity such that the French were "at last able to accommodate to a Socialist Government" (p. 199). Calvert concludes that class is one of W. B. Gallie's "essentially contested concepts" (pp. 214-215), where agreement on a definition is unlikely because of the motives of its users.

In a similar argument about the concept of community, Raymond Plant suggested that definitional consensus is not essential to the productive use of a concept (*Politics and Society,* 1978, *8,* (1), 79-107). Calvert, by contrast, proposes that social scientists dispense with class. Were we to do so, scholars would certainly find themselves more removed from a world in which, as Calvert admits, there is plenty of "classism" (p. 216). That murder has been committed in its name does not distinguish class from race, power, or even, after Machiavelli, politics itself. Do we have so many tools for understanding discontinuities in power and the dynamism of political life that we can abandon class?

The book has intellectual value as a stimulus to the argument about class. The value would be greater if Calvert had gone beyond the familiar positivist critique of Marx and, for example, explored the territory opened up by Bertell Ollman's analysis of Marx's method of defining key concepts in the dynamic terms of "internal relations" with associated phenomena (*Alienation,* Cambridge University Press, 1972). The substantial bibliography is helpful, though there are surprising omissions: Ollman, C. B. Macpherson, Sidney Hook, Alfred Meyer, G. William Domhoff, and Paul Blumberg. The book is engagingly written; the lexicographic analysis and the summaries of different concepts of class are the most useful parts of the book.

KENNETH R. HOOVER

University of Wisconsin-Parkside

The Ethical Dimension of Political Life: Essays in Honor of John H. Hallowell. Edited by Francis Canavan. (Durham, N.C.: Duke University Press, 1983. Pp. vix + 275. $27.75.)

This volume, 17 essays written by friends and former students of John Hallowell, was published upon the occasion of his retirement from Duke University. Sixteen of the essays appear in print for the first time. The volume also includes a list of Hallowell's publications.

As is the case with many commemorative anthologies, these essays do not have the unity of theme that the collection's title implies. While the issue of ethics and politics does surface throughout this collection, in most cases it arises in a secondary fashion, only tangentially related to the more immediate concerns of the various authors. Anyone turning to this book for a comprehensive or systematic treatment of the relationship of ethics to politics will be disappointed.

This is not to say that the essays in this collection are of no merit. A number of them are extremely perceptive and well written. The essays by Kenneth W. Thompson (on foreign policy), Thomas A. Spragens, Jr. (on David Hume), and Mulford Q. Sibley (on coercion) are well worth reading, as are the essays discussed below.

James L. Wiser's "The Force of Reason: On Reading Plato's *Gorgias*" provides an exceptionally clear and concise discussion of the structure of the *Gorgias* and presents a thoughtful argument concerning the relation of intellectual activity to personal life. Wiser argues that "before there can be a true exchange at the level of ideas, there must first be a real encounter at the level of existence . . . intellectual persuasion presupposes an existential conversion" (p. 56).

Claes G. Ryn presents a provocative analysis of Leo Strauss's understanding of natural right and the possibility of a "value-centered historicism" in "History and the Moral Order." He argues that Strauss's understanding of natural right contains a tension between natural right, seen as a set of eternal and universal rules of conduct, and natural right, seen as something mutable because it exists only in concrete decisions. Ryn argues for a position that recognizes the historical context of moral decisions, but that also acknowledges the common experiences of human existence which make moral knowledge possible.

In "The Radical Gospel and Christian Prudence," Clarke E. Cochran deals with the personal and political dimensions of the Christian gospel. The gospel is politically radical because of Christ's identification with the poor and because of his lack of concern for how his message affects political stability. The danger of the radical gospel, according to Cochran, is that it calls humans into a life of continual tension, and a retreat into the security of political ideology becomes an easy way to eliminate the stress of radical Christian living. Cochran sees prudence as an antidote to ideology and argues that the Christian concept of stewardship as the embodiment of prudence provides the proper framework for the radical gospel.

The book's essays move in divergent directions. To the extent that there is a structure to this collection, it is built around the concerns that have occupied John Hallowell in his teaching and writing. Thus there are essays on liberalism, democracy, Christianity and politics, ethics, and a number of detailed explications of individual political philosophers. Unfortunately, most of the essays are little more than preliminary sketches that require more space for adequate development.

As an expression of affection, and as a tribute to the wide-ranging intersts of an outstanding teacher, this volume succeeds. As a contribution to the important area of ethics and politics, however, it is much less successful.

STEVEN D. EALY

Armstrong State College

Feminism. By John Charvet. (London: J. M. Dent & Sons, 1982. Pp. 159. $15.95, cloth; $7.95, paper.)

In this slim, closely written volume Charvet examines theories of the past two centuries of feminist thought. Three sections discuss the major feminist works in the individualist, socialist, and radical feminist traditions. Within severe space constraints, Charvet searches each writer's distinctive contributions, internal inconsistencies, and relationship to the thought of others. A substantial effort is made to be objective, but the approach is analytical, rather than reportorial. The book fills a need and provides a useful framework to debate on feminist issues. Crucial to the approach is the understanding that feminist thought not only arises out of an ethical tradition, but it also becomes subject to the theoretical inadequacies there entailed.

The individualist ethical tradition is based on rights of individuals to freedom from interference. One would expect Locke and Rousseau, advocates of individualism and freedom, to be advocates of women's rights as well. But Locke's theory only softened the previous absolutism of paternal rule, and Rousseau's scheme for the education of women involved a deliberate creation of female dependence. It was left for Wollstonecraft, Fuller, and J. S. Mill to apply individualist concepts to the status of women. All assumed that most women would continue life in the home, although with a new status.

Dissatisfaction with the results of gained legal rights led some individualist feminists in the 1960s to call for societal intervention to equalize resources and status. Such intervention, however, contradicts for Charvet the premise of individual freedom. Friedan's call for child-care facilities indicates to Charvet an unresolved issue. Society must support women as women to make it possible for them to develop as human beings, not as women.

Socialist writers have less trouble than individualists in espousing feminist views of equality, Charvet claims, because the elimination of private property releases women from their bondage to the private family. Both Thompson and Fourier, however, were forced to draw on individualist notions to portray the individual in society. Marx advanced by recognition the unity of individual and society. Feminist consequences of Marxism, developed by Engels and Bebel, and later Gilman, would release women from family responsibilities to find individual/social fulfillment as equal workers and in marriages of unfettered love. Socialists maintained women's distinctive sexual character even in a worker's society.

Radical feminists, in contrast, deny the sexual nature of human beings. Gender characteristics are the creation of society, indeed of man, to protect patriarchal rule. Drawing on a variety of existentialist, historical, Freudian, and New Left themes, such thinkers as de Beauvoir, Figes, Reich, Marcuse, and Greer are proto-radical feminists, pointing to forms of sexual liberation beyond the legal realm of individualists and the economic realm of socialists. In the full development of the radical position, women are seen as an oppressed class. Millett, Firestone, and Mitchell argue that women must become not only economically free, but also sexually free to bring into being an androgynous world. Sexual anatomy will become irrelevant to behavioral characteristics and to the choices of love.

The principle of androgyny does not fit the pattern Charvet has described of feminist theories arising from general ethical systems. Androgyny seems to be a new idea. But, Charvet argues, it cannot stand alone. It requires an ethical theory, individualist, socialist, or some other, to explain the relations of free persons.

In the concluding section Charvet sketches what may be a preliminary to his own position. He appears to reject androgyny, relying on human biological research that points to some biologically determined sexual differentiations. If such differences exist, what is their meaning for socialist or individualist feminists? Perhaps feminine cooperative primacy for the former, or permanent reverse discrimination for women for the latter. Each raises difficulties. It may be, Charvet says, that we must decide how much society needs and values the nuclear family. If it is still valued, it would then have to be shown that some inequality is unavoidable to preserve the family system, and that women have as much public freedom as possible without undermining family values. Is this a return to "Life is not fair"?

MARY ANNE CAVICCHI

John Carroll University

The Feast: Meditations on Politics and Time. By Tom Darby. (Toronto: University of Toronto Press, 1982. Pp. xiii + 234. $30.00.)

The title of this book requires some explanation. "The Feast" is the title of a poem written by the author in Greece in 1969 that serves as the frontispiece of the volume. The title refers to the vision of an age of Arcadian innocence before the emergence of history, a potent symbol throughout Western thought. The subtitle refers to the interruption of that golden age by the advent of politics, history, and alienation. The connecting thread of the work is a rumination on the meaning of history for "post-modern men" attempting to come to terms with the "temporal character" of life in order to restore some semblance of wholeness and unity to their "shattered" and "dismembered" souls.

The substantive chapters of the book follow the contours of modern historicist philosophy beginning with Rousseau and then Kant, Fichte, Hegel, Feuerbach, Marx, and Kojève. Rousseau is credited with being the first to place the problem of history or time at the center of political philosophy. For Rousseau, history was the source of both the grandeur and the misery of the human species. But this essentially ambivalent attitude toward the meaning of history did not appear satisfactory to Rousseau's less cautious heirs, notably Hegel, who was to put it in the service of a confident and progressivist historicism. History could create the community that nature had failed to provide. But the modern state turns out to be highly problematical. For if we are, as Hegel assures us, historical by nature, then the advent of the fully rational social order could only mean the "end of history." Darby seems to agree with Hegel and his interpreter, Kojève, that the famous "end of history" or "post-history" is no longer simply a possibility that might come, but a certainty already present.

Darby's book suffers from a number of flaws.

First, while the problem of time is important, he rarely succeeds in advancing the discussion beyond its present form. Even where his observations are just, he relies on translations and secondary studies that will be familiar to everyone in the field. The treatments of Kant, Fichte, Feuerbach, and Marx in particular are so brief as to be almost negligible.

Second, the book is far too ambitious to complete what it sets out to do. Darby has an admirable love of the big ideas, but he expounds on a variety of subjects—music, painting, literature—that far exceeds the scope of political philosophy even generously understood. A brief glance at a single page (p. 212) brings to light the names of Kafka, Orwell, Mary Shelley, Flaubert, Stendahl, Sartre, Pirandello, Chagall, Richard Strauss, Mozart, Blake, Milton, Thomas Mann, and the Pink Floyd. It is possible that a more skillful writer might have succeeded in tying together all of these diverse strands of the *Zeitgeist;* but as it stands the list appears merely silly.

Finally, having identified the problem—time, history, alienation—it is by no means clear what, if anything, we can or should do about it. Unlike other theorists of contemporary moral disorder (Solzhenitsyn, Strauss, MacIntyre), Darby offers us no clear alternative or way out of the current impasse. Following Kojève's gloomy prognosis, the only prospect he sees is the elevation of a "Japanized" cult of snobbery as a way of asserting human distinction in an essentially "post-historical" age. The solution that Darby offers amounts to nothing short of an admission of political irresponsibility. To speak of "post-history" as indicating the triumph of the "Russo-American way of life" can only mean that Darby finds the principles of liberal democracy indistinguishable from those of Marxian communism. No attempt is ever made to prove this. To imply, as he does, that both are based on the "principle of self-preservation" that reduces politics to a form of "collective housekeeping" is to engage in a species of reductionism that obscures important political differences between East and West. It is just this type of blindness or indifference to political distinctions that I find the most troubling aspect of *The Feast.* At the very least, this book illustrates the dangers of separating the claims of speculation from prudence.

STEVEN B. SMITH

The University of Texas at Austin

Justice, Equal Opportunity, and the Family. By James S. Fishkin. (New Haven, Conn.: Yale University Press, 1983. Pp. vii + 200. $18.95.)

What is to be distributed? How is it to be distributed? These questions of value and structure, respectively, have dominated the recent study of justice. In a welcome change of pace, James S. Fishkin examines the question, "To whom is to be distributed?" This is the question of assignment. Fishkin's procedure is to dissect three central and essential principles of liberalism: merit, equality of life chances, and autonomy of the family. He argues that these principles form a three-cornered dilemma, a "trilemma." Conformance to any two of the three rules out the other. The trilemma results from the nature of the principles themselves and not from the deficiencies of practice. At its most general level, it is conflict of liberty and equality.

The book consists of a systematic exploration of the inconsistencies of the trilemma and an evaluation of possible means of resolution. The discussion ranges over reverse discrimination, Project Headstart, the Coleman Report, the Defunis and Bakke cases, group compensation, and more. Fishkin shows that the trilemma withstands all known solutions. The possibility of relaxing one of the principles to produce a compromise is likewise found wanting. Particularly telling is Fishkin's argument that group compensation sacrifices merit without insuring a gain in equal life chances.

Near the end of the book Fishkin notes Lyndon Johnson's Great Society speech and John Rawls's theory of justice as outstanding examples of the practice and theory of liberalism. Admiring Johnson's vision, Fishkin faults Rawls for accepting the autonomy of the family without spelling out its implications for his theory of justice. Moreover, Fishkin opposes Rawls's rejection of meta-ethical intuitionism, pleading for the toleration of theoretical inconclusiveness where it purchases a grasp on hard choices of political practice. This is a particularly perceptive part of the book.

This is a lucid, informed, and engaging book. Unlike so many others, Fishkin is not content to condemn or praise liberalism but is determined to test its grasp on those hard choices. Not only will the book enlighten its readers, but it will also provoke discussion. In pressing his arguments, Fishkin makes many assumptions that others will delight in questioning. Three instances are worthy of note for their general implications. The first occurs in Fishkin's discussion of Joseph Carens's utopian proposal to socialize into us a concept of social duty rather than consumption as an incentive to earn income. Fishkin opposes this proposal on sensible empirical grounds, but surely the theoretical point is that the scheme substitutes one kind of inequality for another. In Fishkin's terms the scheme represents a change in value not in structure. It is now duty and not consumption that is the source of pleasure, but there is no guarantee that it will be any more equally distributed than consumption is now. Second, Fishkin refuses to take a step outside liberalism's cosmology. Aristotle would say that its theoretical problems are the inevitable result of the misguided attempt to treat all persons alike. Recognizing that people are different, he would say that it would be better to treat them differently according to their individual nature. Now the best theories do not make the best politics, and so one might prefer liberalism regardless of Aristotle, but there is no denying that it is an important point. Finally, Fishkin is satisfied to conceive of the family as an intimate and consensual relationship for child rearing and nothing more. To some this will seem an arbitrary restriction, for one reason because the advantages of a family that interfere with equal life chances are not only intergeneration, but also intragenerational. There is this much—and a great deal more—to talk about in this excellent book.

M. W. JACKSON

Netherlands Institute for Advanced Study

Marxism and Democracy. By Arthur Kiss. Translated by Peter Tamasi and Ivan Sellei. (Budapest: Akademiai Kiado, 1982. Pp. 320. $12.50.)

As the title of this work suggests, the author's primary purpose is the elucidation of the Marxist-Leninist interpretation of democracy. *Marxism and Democracy* is a skillfully reasoned and thoughtfully researched polemic.

The principal merit of the book is Kiss's careful historical exegesis of the democratic concept. The result is a consistently clear contribution to the time-worn debate concerning the definition of democracy, a debate often characterized by terminological confusion and transparent propaganda.

Kiss argues that there are three essentially different interpretations of democracy. Chronologically, the first interpretation, discussed by both Plato and Aristotle, focuses on democracy as a *"form of rule."* The primary emphasis is on *"which social stratum is ruling within the society"* (p. 19). Contrasted with an oligarchy, in which the exercise of power is limited to a propertied elite, democracy implies governance by the propertyless majority. Appropriated centuries later by the Jacobinic dictatorship, this philosophy also underlies the Marxist-Leninist concept of democracy.

A second interpretation, whose historical antecedents can also be traced to the time of Pericles, was given its modern form by such writers as Spinoza, Alexis de Tocqueville, and the ideological representatives of the bourgeois class. This interpretation of democracy focuses on forms of government, limiting "the meaning of democracy to a way of choosing a government and holding it responsible to the people for its actions" (p. 28).

Yet a third interpretation (and from Kiss's point of view the least significant) broadens the concept of democracy beyond the sphere of politics per se. From this point of view democracy becomes synonomous with a way of life: "democracy = humanism,' 'democracy = civilization,' 'democracy = freedom,' etc." (p. 31). "This interpretation started to spread during the First World War and in the period immediately following the war. It figured primarily as the propaganda slogan of the Entente powers who 'in defense of democracy' attacked the 'tyranny' and 'barbarism' of the Central Powers" (p. 31).

Not only is this last interpretration of democracy the least widespread, according to Kiss, it is also the interpretation fraught with the most inconsistencies and vagaries. For Kiss therefore, the most fundamental theoretical controversies concerning the meaning of democracy involve the differences between the class relations characteristic of socialist versus bourgeois democracy. The balance of *Marxism and Democracy* is devoted to an iconoclastic analysis of the limitations inherent in the bourgeois understanding of democracy.

In Part 1, Kiss endeavors to expose the hollowness of the most revered bourgeois-democratic verities by revealing the hidden class biases on which they are based. In particular, he assails the antimony between democracy and dictatorship and the corresponding identification of democracy with freedom so prevalent in Western democratic thinking. In Part 2, a discussion of the "criteria of democracy," Kiss argues similarly that it would be foolish to maintain that any general criteria of democracy exist that transcend the determinations of class structure. He argues throughout for the superiority of Marxist-Leninist analysis and socialist practice.

Unfortunately, Kiss's analysis of socialist democracy lacks the critical insight evident in his discussion of bourgeois democracy. He accepts without question the fundamental principles, not only of Marxism-Leninism, but also of the C.P.S.U. This tendency is most pronounced in Part 3, "On the Essential Features of the Democratic Exercise of Power." Not only does Kiss fail to question the democratic structure of C.P.S.U., but he also accepts, without apparent reservation, pronouncements of the Party as descriptions of fact. Kiss's ideological biases, which fuel his provocative critique of bourgeois democratic formalism, limit his insight into the weaknesses of socialist democracy.

RONALD J. HUNT

Ohio University

Rousseau after Two Hundred Years: Proceedings of the Cambridge Bicentennial Colloquium (1978). Edited by R. A. Leigh. (New York: Cambridge University Press, 1982. Pp. xv + 299. $59.95.)

No imaginable collection of commemorative essays could do full justice to Jean-Jacques Rousseau: the greatest of all critics of inequality, the purest social contract theorist of the eighteenth century (and simultaneously the deepest critic of contractarianism after Hume), the greatest writer on civic education after Plato, the most perceptive understander of mastery and slavery after Aristotle and before Hegel, the finest critic of Hobbes, the most important predecessor of Kant, the most accomplished didactic novelist between Richardson and Tolstoy, the greatest confessor since St. Augustine, the author of paradoxes ("the general will is always right" but "not enlightened") that continue to fascinate or infuriate. Nonetheless, the present group of 14 articles edited by R. A. Leigh, the distinguished editor of Rousseau's *Correspondance Générale,* contains at least three essays that successfully illuminate Rousseau's greatness and several others that are worth reading.

Although it is controversial, the finest of the essays is John Charvet's "The Idea of Love in *La Nouvelle Héloise."* Extending the argument of his brilliant *The Social Problem in the Philosophy of Rousseau* (Cambridge University Press, 1974), in which he insisted that Rousseau's obsession with *généralité* led him to the "absurd" neglect of real persons in their "particularity," Charvet now urges (p. 140) that "the ideal of pure love [in *Héloise*] is an abstraction from the particular natures of the lovers. . . . Since men and women have particular natures . . . it is obvious that the ideal as such as unrealisable" and necessarily yields unhappiness. But for Charvet, Rousseau's misconception of "ideal" or nonsexual love in his novel merely reflects a larger Rousseauean problem: "his abstract idea of the citizen is as repressive and destructive of particular life as is his idea of the pure lover" (p. 145). Whatever one makes of this, it ingeniously pulls the (otherwise somewhat isolated) *Nouvelle Héloise* into the very heart of Rousseau's politics.

Equally successful are two French essays: Bronislaw Baczko's *La Cité et ses languages* and Jean Starobinski's *Rousseau et l'eloquence.* Baczko carefully shows the paradoxical place of language in Rousseau's political thought: The Great Legislator of *Du Contrat Social* who must transform particular and particularistic men into citizens with a "general will" can resort only to persuasive language, since force is illegitimate; but a people, having arrived at civic *généralité* and perfect unity, will manifst what Rousseau calls a *"silence universelle"* in which language withers owing to perfect harmony. In Baczko's Rousseau, language is at once supreme and negligible. Starobinski's essay divides Rousseau's celebrated eloquence into an "indignant" part and a constructive part, into the *accusation négative* of *Inequality* and the "systematic" *Contrat Social* and *Émile.* But even in *Émile,* as Starobinski rightly says, "between the accusation that culpabilizes the reader, and the affecting word that aims to gain his heart and his faith, the alternation is often rapid."

Less valuable but interesting is Stephen Ellenburg's essay "Rousseau and Kant," in which he accuses Rousseau of abandoning an anarchistic self-government of "radical" equals in favor of a Great Legislator whose educative activities Rousseau rationalizes through "specious reasoning." Since Ellenburg insists on reading Rousseau as an anarchist, he can hardly fail to see the Legislator as an intolerable lapse; may it not be, however, that the Legislator brings the people to ultimate "enlightenment," so that morally autonomous citizens can finally (at the end of civic time) say, with Émile, "I have decided to be what you have made me"? And Ellenburg's constant insistence on "compulsion" and "coercion" in Kant's political thought—seven times in one page—utterly neglects Kant's central political concerns: universal republicanism and infinite approximation "towards eternal peace" (*zum ewigen Frieden*). (Jean-Louis Lecercle's *Rousseau et Marx* alternates between accusing Rousseau of "idealism," "ideology" and prescientific "utopianism," and praising him as a pre-Kantian who treats men as ends, never as means only.)

Limited space rules out any discussion of good (but not centrally political) essays by Bernard Gagnebin, Michel Launay, and Ronald Grimsley. However one cannot fail to mention the reappearance, in the shape of an essay called *"Rousseau et la morale du sentiment, (lexicologie, idéologie),"* of the venerable J. S. Spink, who gave us the first publication of the original manuscript of the *Lettres écrites de la montagne* more than a half-century ago.

PATRICK RILEY

University of Wisconsin (Madison)

Social Theory and Political Practice. Edited by Christopher Lloyd. (New York: Oxford University Press, 1983. Pp. ix + 182. $33.00.)

In his introduction to this collection of the 1981 Wolfson College lectures, Christopher Lloyd states that the essays represent a new examination of the old question of the relationship between social theory and political practice in the wake of the demise of both positivism and functionalism. He informs the reader that the collection will deal with the question of whether shared objective social knowledge is a possibility. To illustrate the way in which philosophical ideas about the nature of social understanding can have a significant effect on the perceptions of the political process, he recapitulates the Popper-Habermas debate that took place at the 1961 German Sociological Association meeting. Bringing in the view of Gadamer as well, Lloyd argues that the philosophical positions of the three theorists dictate distinctly different political practices.

Lloyd's introduction is problematic in two respects. First, issues debated in the Popper-Habermas dispute are no longer central to contemporary discussions in social theory. Second, issues actually discussed in the majority of the essays deal with the relationship between classical social theories (for example, Marx, Weber, and Durkheim) and contemporary political practice. Epistemological issues, so much in the forefront of contemporary discussions in social theory, are rarely mentioned. And in the two essays that do deal explicitly with current issues in social theory, those of Charles Taylor and Amartya Sen, there is little relationship between these discussions and political practice.

These reservations aside, however, the collection offers some valuable, if not entirely innovative, discussions of the topic. The perspectives of the authors is varied: David Marquand and John Dunn discuss issues central to a liberal perspective; Wlodzimierz Brus examines the relationship between Marxism and contemporary communist societies; Ralf Dahrendorf and Tom Bottomore explore the implications for political practice of the theories of the founding fathers of social theory. A number of noteworthy insights arise from these discussions. First, both Dahrendorf and Bottomore reevaluate the commonly held assessment of the positions of Marx and Weber. While Marx is usually interpreted as attempting to unify theory and practice, Weber is commonly seen as rigidly separating the two. The authors argue that these assessments both distort the thought of Marx and Weber and present an unworkable approach to the relationship between theory and practice. A second virtue of the collection is the evenhanded assessment of the relevance of Marxism for contemporary communist systems offered by Wlodzimierz Brus. Avoiding the dogmatism of many Marxist discussions of this issue, Brus argues that, with some necessary alterations, Marxist theory can offer concrete guidance for the current problems of communist societies.

Perhaps the weakest essays in the collection, ironically, are those that deal explicitly with contemporary problems in social theory. Although Charles Taylor offers a well-argued defense of the civic humanist position, he falters when he attempts to offer a criterion for the "rightness" of social theories. The criterion he proposes is vague and ill-defined: It involves asking to what extent theories transform political practice. In contrast, Amartya Sen offers what appears to be a neo-positivist account of social theory and political practice resting on a dubious distinction between actions and accounts. His chapter reveals the weakness of even a modified version of the positivist approach to this issue.

This collection is worthwhile reading for those who are interested in the broad issue of social theory and political practice, although not for those concerned with the details of contemporary social theory. Unlike many collections of its kind, the essays remain fairly close to the stated topic. And, although it does not fulfill Lloyd's promise of providing a truly new examination of the topic, it does represent a contribution to the ongoing discussion of the issue.

SUSAN HEKMAN

University of Texas at Arlington

Prejudices: A Philosophical Dictionary. By Robert Nisbet. (Cambridge, Mass.: Harvard University Press, 1982. Pp. 318. $17.50.)

Robert Nisbet ranks among the chief figures in the postwar revival of conservative thought. During the past three decades, Nisbet has written with a nearly Cassandra-like foreboding that the moral and social prerequisites of a genuinely pluralistic community are gravely endangered by the encroachments of modern collectivism. Employing a seldom-utilized literary form, he succinctly summarizes here the positions and principles with which he has long been associated.

While the form of *Prejudices* makes comparison with Voltaire's *Philosophical Dictionary* or Mencken's *Prejudices* series unavoidable, the principles of Burke and de Tocqueville, not those of Voltaire and Mencken, loom large in these pages. As Nisbet observes in his brief Preface, *Prejudices* is intended to be "neither philosophical nor a dictionary," but rather as a vehicle to express mordant social, moral, cultural, political, and economic criticism. In a collection of brief essays on 71 topics arranged alphabetically, ranging from abortion, golden ages, and old age, to authoritarianism, ideology, judicial activism, progress, reification, war, and wit, Nisbet expresses his "personal observations, likes, and animadversions." Nisbet's personal tastes apparently determined the selection of topics.

"Prejudice" here does not imply arbitrary judgment, but rather is used in the Burkean sense to signify a prejudgment that incorporates the collective and immemorial wisdom of a race. Good traditions, and hence beneficial prejudices, will check the selfish and turbulent passions of a people. "Prejudice" also suggests a distrust of a certain type of narrow reasoning inspired by the eighteenth-century Rationalists, which challenges the moral authority of traditional institutions, mores, and customs.

Nisbet strongly prefers those principles associated with the conservative tradition to those of its ideological adversaries. The conservatives stress "family, neighborhood, local community, and region" against the demands of the centralized state (p. 55). By contrast, Nisbet condemns liberals as "ardent advocates of the kind of power that is resident in the national state." Furthermore, as a "lover of political power," the liberal is "the knee-jerk adversary of all moral authority" (p. 211). In spite of the recent political successes of conservatism, Nisbet is not at all sanguine about its future in the United States. He predicts that the conservative momentum soon will be shattered by (1) internecine factionalism; (2) some "self-styled conservatives," oblivious to the traditional conservative defense of the social order against the infringements of the state, "more interested in capturing the state, or part of it at least, as the means of imposing a given moral value upon the entire nation"; (3) a "rising tide of militarism"; and (4) "the sheer mass of the liberal provider-state" which dwarfs all efforts to reduce it (pp. 59-61). The future of liberalism, hence, he concludes, "is very bright, at least in the short run" (p. 216).

Nisbet's animosity to Rousseau and the modern manifestations of his principles parallels that formerly felt by Irving Babbitt, who had charged that Rousseauistic humanitarian morality was nearly single-handedly responsible for the decay of all that was best in Western civilization. The cult of the noble savage and the growing hostility to traditional institutions, inspired by Rousseauistic notions, have done much in Nisbet's opinion to shake the moral moorings to which people cling in community and thus have opened the way to the total political community into which the individual is immersed (p. 54).

On many social and political issues, Nisbet's opinions will clearly outrage many readers. He vigorously challenges some of the most cherished contemporary convictions held by many in the academic community. He describes environmentalism as "the third great wave of redemptive struggle in Western history," the other two being Christianity and modern socialism (p. 101). Because they have accepted the erroneous Rousseauistic belief that nature is benign, Nisbet accuses the environmentalists of seeking to annihilate all the benefits of modern technology (p. 105). Futurology represents "one of the more pretentious of the pseudo-sciences of the twentieth century" (p. 131), while psychohistory is dismissed as an "offensive practice in the contemporary writing of history" (p. 246).

Like Mencken's *Prejudices,* Nisbet's opinions on the more pressing problems of our times will entertain, provoke, annoy, and please. Frequently witty and controversial, Nisbet's insights never fail to stimulate and inform. *Prejudices* should be placed on a shelf in easy reach near such great essayists as Montaigne, Twain, Mencken, Emerson and others to whose insights and wisdom we often feel the need to repair.

W. WESLEY McDONALD

Elizabethtown College

Instrumentalism and American Legal Theory. By Robert Samuel Summers. (Ithaca, N.Y.: Cornell University Press, 1982. Pp. 295. $24.50.)

Robert Samuel Summers's *Instrumentalism and American Legal Theory* is a detailed summary of and critical commentary on the leading tenets and "general directions of thought" that characterize the American jurisprudential movement labeled by Summers "pragmatic instrumentalism." The object of his study is the product of the coalescence of philosophical pragmatism, sociological jurisprudence, and legal realism as manifested in the pre-1940 work of legal theorists such as Holmes, Pound, Gray, and Llewellyn. Summers states that his purpose is not primarily historical, but is rather "the exploration of the instrumental and pragmatic facets of legal phenomena as such" (p. 12). By choosing to accomplish this by means of an examination of American writers, Summers pays tribute to the central role played by Americans in the development of pragmatic instrumentalism and hopes to establish their work as a "fourth great tradition" of Western jurisprudence, alongside the traditions of natural law, analytical positivism, and historical jurisprudence (p. 13; cf. pp. 19, 268).

The largest portion of the book is devoted to a characterization of pragmatic instrumentalism, the chief features of which are: (1) a "theory of value" that is "utilitarian, quantitative, conventionalist, and majoritarian in tenor" (p. 42); (2) a "technological" view of law that sees it as "a set of means to be used in the service of social goals (which, in turn, derive from conventional wants and interests)" (p. 61); (3) a commitment to the methods of the natural sciences, with law as a "technology" and legal personnel as "social engineers" who, when armed with scientific method, could "resort to laws of social causation and to laws of legal causation to predict the efficacy of alternative legal means, to determine costs and benefits, to choose the preferred means-goal hypothesis, and to implement it" (p. 89). This approach was prescribed not only for legislators, but also for courts; (4) a "source-based theory of validity" that measured the "validity" (legitimacy?) of a law entirely on the basis of its having an authoritative positive source. Indeed, law is seen as "a set of predictions" of what authorized officials (usually judges) will do. As Holmes put it, "the prophecies of what the courts will do in fact, and nothing more pretentious, are what I mean by the law" (p. 117); (5) a rejection of "formalistic" modes of legal interpretation with their emphasis on "coherence, harmony, and consistency with existing law" (p. 157), and a corresponding emphasis on particulars ("rule skepticism"), legal (including judicial) creativity, and, in general, "law in action" as opposed to "law in books"; (6) a radical separation of law and morals; and (7) an elevation of "personnel" over rules and structure as the primary determinants of a legal order, accompanied by an emphasis on coercion and force.

The remainder of the book consists of an exposure of the shortcomings of the views of the pragmatic instrumentalists and suggestions of how to supply those defects so as to make instrumentalism "somewhat more intelligible and coherent than it has been previously" (p. 13). It cannot be said that Summers succeeds in providing "a comprehensive framework" for the continuance of his project (p. 13). Instead, what he has accomplished is the disclosure of the dubious and often self-contradictory character of most of the doctrines and assumptions held sacred by the pragmatic instrumentalists. (It must be noted that much of Summers's argument in this regard retreads ground already covered in Gary J. Jacobsohn's *Pragmatism, Statesmanship, and the Supreme Court* (Cornell, 1977). Summers's book is interesting, then, primarily for its destructive, not its constructive, aspects.

Summers's critique is most telling in the realm of constitutional adjudication. The experimentalist strain of pragmatism strikes at the very foundation of American constitutionalism, grounded as the latter is on certain substantive conceptions of justice and dedicated to the rule of law, both of which imply a degree of fixity and continuity in the law unacceptable to the pragmatists. The incompatibility of American constitutionalism with pragmatic instrumentalism appears starkly when one compares the natural rights jurisprudence of the Founders with Holmes's standard of legal justice: "I am so sceptical as to our knowledge about the goodness or badness of laws that I have no practical criterion except what the crowd wants" (p. 46). Summers's weak denial that "extreme skepticism about value" is intrinsic to "any of the general directions of instrumentalist thought" (p. 52) is akin to an assertion that Hobbes's relativism is unrelated to his strong conventionalism, and indeed, his authoritarianism.

Despite his intention of saving pragmatic instrumentalism from itself, Summers has written a persuasive brief against its continued existence. His search for an alternative American jurisprudence, however, will most likely end in futility unless he jettisons the parochialism of the law schools that continue to teach that the work of Pound, Holmes, et al. "qualifies as America's only indigenous legal theory" (p. 12). The startling narrowness of that teaching is manifest in light of Marshall's outline in *Marbury* v. *Madison* of a "theory" that is "essentially attached to a written constitution," which constitution "we have deemed the greatest improvement on political institutions." Summers's book gives us every reason to believe we have much to learn from a reconsideration of the Framers's intent.

BRADFORD WILSON

California State College, San Bernardino

Political Reasoning. By Evert Vedung. (Beverly Hills, Calif.: Sage Publications, 1982. Pp. ix + 232. $21.25.)

Vedung states that, although there used to be rational analysis (p. 16), it is currently by definition incorrect in view of our latest findings in epistemology. He continues that form of argument as he attempts to classify analyses as functional and content-oriented, descriptions that represent strict Aristotelian categories. Vedung does, however, conclude that the categories' boundaries are fuzzy and not demarcated, a conclusion that does not totally rectify the matter, since no alternative is suggested.

For those interested in strict categories and static language, this book may prove to be a significant contribution, but for those who seek a more fluid kind of process, the book may seem restricting and stifling. It is a good representation of Aristotelian thinking, which promotes rigidity and nonflexibility in our thinking. The book could be classed as part of the second conceptual revolution, which gave rise to most of our present common-sense thinking. However, since it was not meant to be a book on innovative thinking and new paradigms, it compares favorably to books that represent categorical thinking.

Topics covered in the book include: concept formation, mutual exclusiveness, schemes of classification, kinds of interpretations, the principles of clarity, Rawls's theory of justice, unconscious and conscious obscurity, first-order and second-order disagreement, the essentialist view, relevance, testing truth, and appraising values.

Chapters 4, 5, and 6 introduce psychological, philosophical, and epistemological concerns focusing on the ideas of Fichte, Popper, Marcuse, Bentley, and Truman.

The discussion of intension and extension (pp. 70-71) would be helpful if they were treated in greater detail. To put too much emphasis on the differences between "vagueness" and "ambiguity" (pp. 72-78) does a disservice to an important discussion.

The chapter on topic selection, problem formulation, and concept formation is somewhat different; the subject matter is rather sophisticated. On page 60 there is an important focus on language, although not much innovative is said. In other words, there is no examination of symbols, which would be necessary to give the topic depth.

Since language symbols provide a microscope for the social sciences, a more intense concentration on such symbols would have unified the book. While this was alluded to on page 14, the approach has been confined to words and their obscureness, definitions as a technique, and making distinctions between such things as stipulative definitions and reportative definitions. On a superficial level of analysis, this might satisfy the need for analytic reporting in order to keep things simplified and within the realm of common sense. In order to communicate a theoretical model that would enable us to understand complex situations, a method of inquiry is necessary that would raise questions about the development of symbols, promote an understanding of language as a conceptual tool for formulating theories, enable us to analyze the differences between static and nonstatic terminology, and equip us to develop the nonstatic differences that produce the best results. Unless this approach is taken, an overriding thesis for examining holistically cannot be provided. Instead, we are confined to a reductionist approach.

Vedung's book is about how to apply the language to understanding political affairs, using "splices" of epistemological background data to give meaning to mechanical methods of inquiry. The theme is largely of an either-or nature, which provides for a limited understanding of complex political questions.

WILLIAM J. WILLIAMS

University of Southern California

Spheres of Justice: A Defense of Pluralism and Equality. By Michael Walzer. (New York: Basic Books, Inc., 1983. Pp. xviii + 345. $19.95.)

Michael Walzer has written a subtle, literate, and at times brilliant book. His purpose is to describe an ideal society organized on the principle of what he calls "complex equality": The distributive rule is that no social good x should be distributed to men and women who possess some other good y merely because they possess y, without regard to the meaning of x. Walzer rejects utilitarian and rights based distributive theories of justice. He believes that the distribution criteria should be contingent upon social conventions found in a society.

Through the brilliant use of historical and comparative examples Walzer describes what American pluralism might be like if the 11 spheres of justice and their distribution criteria, which he identifies, were autonomous of each other: (1) "Membership" determined by the political community consisting, at least, of all those who live and work in its territory; (2) "Security and Welfare" determined on the basis of need with level of welfare chosen by the community; (3) "Money and Commodities" distributed by the economic system on the basis of the skill and luck of those active in it; (4) "Office" (bureaucracy) filled or regulated by public officials to be distributed on the basis of talent for the task to be performed, unskilled work on the basis of other criteria; (5) the distribution of "Hard Work" to be the basis of redefined criteria such as worker participation and management, higher wages, or national conscription to add to societal respect for such tasks; (6) "Free Time," (7) "Kinship and Love," and (8) "Divine Grace" distributed, respectively, on the basis of individual choice, loving and familial ties, and respect for God as decided by the individual, the family, and ecclesiastical bodies, with minimal interference by the political community; (9) "Recognition" distributed by juries on the basis of objective review of performance or behavior; (10) basic "Education" at a level high enough to prepare all for citizenship distributed on the basis of "simple equality," higher education, broadly defined, available to all on the basis of talent and interest; and (11) "Political Power," the most significant sphere of justice, to be distributed through election of those with debating and coalition building skills.

Walzer argues that tyranny and injustice occur when criteria of distribution are used in spheres to which these criteria do not properly pertain as determined by the social conventions and political community. He feels that autonomy of sphere will lead to individual self-respect, legitimacy for the society, and greater social harmony, in part because individual deserts will mirror individual performance, and the political community will have a means to decide the nature of the deserts.

The sphere of political power will be the forum to decide the distributive criteria in all spheres, although it will be limited by constitutional principles and social convention calling for public private distinctions in such spheres as Divine Grace and kinship and love. Elections would be on a one-person-one-vote basis. However, degree of political power would be based on influence within a party system in focused policy discussions. That influence is to be based on rhetorical and leadership qualities, not economic resources, recognition, office, or divine grace. "Complex equality" requires equal opportunity to be active in politics after *direct* use of social goods gained in other spheres is blocked.

Walzer's concern for freedom (and noncoercive government) and his failure to offer detailed analyses of the connections between the political sphere and the other spheres and among institutions in the political sphere make his argument unconvincing. I question whether sphere autonomy alone, as described in this book, will produce both the equality and pluralism that he desires for American society. Moreover, there is no way to prevent an unequal distribution of "debators" from particular spheres from dominating the political sphere. There is a good chance that the political system will continue unequal reproduction of spheres, perhaps to a somewhat lesser degree than today. However, the rules against reiteration of the class or racial makeup in distribution in such spheres as recognition, welfare, and office will make it more difficult to change the nature of stratification systems. This reality will not lead to the social harmony that Walzer desires.

Furthermore, Walzer offers us no help in establishing process norms by which to choose the social criteria for viewing a problem even if we assume the sovereignty of the political community. There is not even a ranking of spheres other than political power with regard to when one sphere should be chosen over another. Nor is there any help with the question of the role of participatory bodies versus federal courts in deciding issues in light of his goal of sphere autonomy.

Finally, in arguing for localism when the national community cannot agree on social norms and for as much decentralization of authority as constitutional values allow, Walzer fails to consider the effect of size of political unit and its make-up on how issues are debated and on the manifestations of equality as a social criteria. At a minimum, these issues should be addressed in this process view of distributive justice. They would have to be considered irrespective of the nature of the social norms in a society and its contingent spheres of justice, for they are at the center of the question of whether equality, not merely pluralism, will be realized.

RONALD KAHN

Oberlin College

The Wilderness and the City: American Classical Philosophy as a Moral Quest. By Michael A. Weinstein. (Amherst: University of Massachusetts Press, 1982. Pp. x + 162. $17.50.)

The theme of this slim volume is straightforward: Does American classical philosophy provide contemporary people with an adequate guide to action in the public world?

The question is rather ambiguously answered in *The Wilderness and the City*. On the one hand, Weinstein is convinced that Josiah Royce, C. S. Peirce, William James, John Dewey, and George Santayana accepted the challenge of Nietzsche's "death of God" thesis. On the other, he is unhappy with the shallowness of their response to that encounter.

Common to the classical American philosophers, Weinstein argues, was their attempt to wallpaper over the philosophical and psychological implications of *acedia,* a "sense of loneliness, forlornless, and solitude that crushes the will to strive" (p. 7). For the most part, he notes, these philosophers substituted a new faith in the possibility of creating a moral human community for an earlier faith in an individualism denied the comfort of a universe of fixed and settled meanings.

"The severest criticism that can be leveled against American classical philosophy," Weinstein observes, is that when confronted with the consequences of the death of God, "no substitutes for God, whether cultural, social or psychological are acceptable" (pp. 135-136). More acceptable, apparently, is William James's acknowledgement that the encounter with nothingness is "the gateway to empiricism" and inner tolerance (p. 136), that is, that self-control is an adequate surrogate for social action.

Weinstein's analysis of the limits of the American classical philosophical tradition proceeds with surety and careful scholarship, reaching its most incisive moments in his discussion of James's and Dewey's thoughts.

When faced with the void, he suggests, James's great contribution to American life-philosophy was his insistence that being itself is good (p. 85), and that faith in life is an aid to living in the present. According to Weinstein, however, James's weakness was his very optimism that was never quite able to cover over the terror of a purposeless universe.

As for Dewey, Weinstein holds "his effort to determine how control over nature could be used well" (p. 101) was marred by his misplaced confidence that culturally and philosophically demeaned individuals were capable of acting morally on the basis of shared emotions.

While *The Wilderness and the City* frequently sparkles with striking images and elegant insights into the ideas of America's early twentieth-century philosophical greats, Weinstein is less successful in offering a remedy for today's encounter with the abyss. On a planet where all centers of meaning are now exposed to extinction, it is doubtful that the development of self-control (individualism) to the point where it is "not eccentric to live with world-sickness and still be free for commitment to limited tasks within the public domain" (p. 155) is a sufficient balm to both a philosophical and empirical threat to existence.

In such a setting, it seems much more sensible to combine conservative philosopher Irving Babbitt's call for individuals to restrain themselves in amassing and using power with Dylan Thomas's advice to his dying father: "Do not go gentle into that good night, . . . Rage, rage against the dying of the light."

The final strength of the classical American philosophers, particularly James and Dewey, was and is their understanding that without philosophical risk and active commitment to the continuance of the moral community, purposeful life and world survival are impossible and, perhaps, undesirable.

MARK READER

Arizona State University

Empirical Theory and Methodology

The Anatomy of Racial Attitudes. By Richard A. Apostle et al. (Berkeley: University of California Press, 1983. Pp. x + 331. $27.50.)

It is fitting that Richard Apostle, Charles Glock, Thomas Piazza, and Marijean Suelzle's study of whites' racial attitudes should be published in 1983, 20 years after Martin Luther King challenged us to share his dream of racial equality. Readers will naturally ask to what extent the study contributes to the achievement of Dr. King's dream. They will find that this book has virtually no relationship to that dream. Instead, it is a narrow contribution to the social indicator movement that was important in social science two decades ago.

The Anatomy of Racial Attitudes reports the results of a project in social indicator construction. The object of the project, which is based mainly upon data collected in 1973, was to develop empirical indicators that could be used to describe white Americans' attitudes about their black countrymen. This limited purpose justifies the use of a sample design that was limited geographically to the San Francisco Bay area, but was not truly representative of even that population.

The authors reject "prejudice" as a useful concept because it narrows the scope of any inquiry into "racial attitudes." The latter, more neutral, term is used throughout the book.

Similarly, the authors reject the standard description of the components of attitudes along cognitive, affective, and behavioral dimensions. Instead, they set out to describe racial attitudes along three other dimensions: perceptions of differences between racial groups, explanatory modes of racial attitudes, and prescriptions for change.

The major part of this study is devoted to developing and applying what the authors refer to as the explanatory modes of racial attitudes. They wisely choose to focus upon prescriptions for change rather than behaviors. This permits them to observe how different perceptions and explanations lead to support or lack of support for various governmental programs aimed at achieving true racial equality. However, the chapter dealing with the relationship between prescriptions and explanations is the weakest in the book.

The study's strength lies in its conceptualization of racial attitudes. According to the authors, the most significant finding of the study is the conceptual and empirical development of the typological explanatory-mode variable. Racial differences tend to be explained by reference either (1) to individual responsibility—Afro-Americans' lives are different than whites' because the Afro-Americans choose to live differently; (2) to genetic differences between races—implying that any action to achieve equality is futile; (3) to supernatural forces or "God's will"—implying that action to achieve equality is probably blasphemous as well as futile; (4) to social or environmental forces—which are clearly subject to manipulation by policy; (5) to elite or other manipulation which the authors describe as a radical explanation for racial differences; or (6) to some combination of two or more of the pure modes of explanation. About 50% of the Bay Area residents surveyed employ one of the five pure types of explanation. The rest use some combination of modes to explain racial differences.

Parts of the book may be valuable in graduate research methodology courses as a source of examples of indicator development. However, the limited findings of the study are mostly in the conceptual area—the idea that racial attitudes can be explained with a typological instead of a continuous variable.

The book is based upon extensive data analyses. However, any conclusions drawn from those data can apply only to the Bay Area and, given the research procedures, their application to that population is questionable. (In fairness, I must add that an exercise in concept development does not require the same sort of sample design that one would look for in a descriptive study.) There is virtually no reference to crucial public policy issues in this study which is obviously fitting here given the authors' focus on development of social indicators.

The conceptual conclusions from the study along with their empirical support could easily be reported in one or two articles instead of more than 200 pages of often-redundant text followed by about 100 pages of appendixes. Chapters 1, 4, and 5 are the most valuable; however the book is an exercise in concept development, based upon a geographically unique and somewhat questionable sample design. A report based upon decade-old data cannot be thought of as especially timely.

JAMES W. HOTTOIS

Eastern Oregon State College

The Foundations of Policy Analysis. By Garry D. Brewer and Peter deLeon. (Homewood, Ill.: Dorsey Press, 1983. Pp. xvi + 476. $24.00.)

The Logic of Policy Inquiry. By David C. Paris and James F. Reynolds. (New York: Longman, 1983. Pp. xii + 276. $25.00, cloth; $12.95, paper.)

Political science's most rapidly growing subdiscipline surely must be the one concerned with public policy studies. Yet it also appears to be the discipline's most ill-defined undertaking. The virtue of these important new books by Brewer and deLeon and by Paris and Reynolds is that they provide valuable perspectives on the fundamental enterprise involved in the cross-disciplinary as well as sub-disciplinary policy studies movement. Stated most simply, these books should be required reading for anyone interested in the study of public policy, whether that interest is captured by the label public administration, policy studies, policy analysis, policy science, comparative public policy, or some other term.

Paris and Reynolds deal effectively with the topic of the logic of inquiry for policy analysis. Most of the book is devoted to characterization, analysis, and criticism of three major traditions of social inquiry and examination of their relevance for policy analyses, although much of what is said is relevant for the examination of how policy is made and implemented. The tradition of behavioralism is portrayed as having a close association with logical positivism and as involving the use of the scientific approach to the study of public policy, with the emphasis on theory development, hypothesis testing, observation, and measurement. The economic tradition is characterized as emphasizing analyses based upon a model of human rationality as well as stressing theoretical assumptions, utilitarian principles, Pareto optimality, and analytical strategies such as cost-benefit analysis. Interpretive policy inquiry is seen as emphasizing the purposes and intentions that motivate policy actions and involves use of various methodological orientations based upon phenomenology, critical theory, Marxian, and neo-Marxian ideas. Paris and Reynolds provide thoughtful discussions of the advantages and limitations of each of these traditions in public policy analysis. They also offer their own approach, which emphasizes the logic of a "rational ideology" and builds upon certain elements of the behavioral, economic, and interpretive traditions.

Brewer and deLeon provide an impressive overview of the development and current status of an approach to the study of public policy that most readers will identify as the policy science approach. They build explicitly on the ideas of

Harold D. Lasswell (even dedicating the book to him) and organize the material around six phases in the policy process. The initiation phase focuses on problem recognition and context and on the generation of policy alternatives. The estimation phase emphasizes the estimation of consequences of policy alternatives, and the presentation is excellent in providing interesting perspectives on complexity, models, methods, and data. The selection phase involves choice among alternatives, and the discussion appropriately focuses upon matters such as the role of information, knowledge, and compromise. The implementation phase comes next, and the presentation emphasizes theoretical paradigms as well as case studies. Evaluation and termination comprise the last two phases in the policy process, and the discussion focuses upon goals and criteria and also emphasizes the need for further development of these activities. A very impressive list of suggested readings is presented by the authors.

There are, to be sure, things to quarrel with in each of the books. For example, it seems to me that Paris and Reynolds present a narrow conception of the behavioral approach to policy analysis, and the Brewer and deLeon statement regarding evaluation strikes me as being somewhat incomplete. However, inadequacies do not mar seriously the contributions of the authors in focusing attention on various important aspects of the study of public policy. In general, the authors of both books are systematic, perceptive, and, above all, provocative in making stimulating presentations. These books clearly represent steps in the right direction and make contributions toward bringing coherence to the chaotic enterprise of the study of public policy. If you choose to read only two books this year that deal with the study of public policy then you will not go far wrong in choosing these two books.

FRANCIS W. HOOLE

Indiana University

A General Theory of Exploitation and Class. By John E. Roemer. (Cambridge, Mass.: Harvard University Press, 1982. Pp. 298 + xiii. $27.50.)

This extremely ambitious and important book is as controversial as any work in contemporary political economy. It is a book that should be read, discussed, and argued about. Roemer provides an alternative to the traditional—and tired—ways of thinking about such significant issues as exploitation and class. Even those who ultimately detest his approach will benefit from considering it.

My claims for the book's importance rest primarily on Roemer's formalization of Marxian political economy. He brings Marxism into the second half of the twentieth century, no longer permitting its easy dismissal as the radical critique of the world before the Russian Revolution. Roemer argues that the underlying premise of historical materialism is the understanding of economic inequality and surplus appropriation; from these factors can be deduced political behavior and superstructural phenomena. His approach is to focus on the variation in institutions that produce exploitation. Marx wanted to account for the existence of exploitation once labor exchange was no longer coercive. Roemer wants to account for the existence of exploitation when the means of production are no longer privately owned. First, he offers a model derived from the labor theory of value. Later, he puts forward a game-theoretic model that relies on counterfactual alternatives to the property relations of the society under investigation. It is this model, derived from a property rights approach, that he claims as his general theory.

Roemer gives that over-used, much-abused term exploitation concrete meaning. In the process he makes clear that gains from trade and exploitation are not mutually exclusive. His formulation of exploitation also enables him to demonstrate that class position is determined endogenously in the model, if one assumes—as he does—that individuals optimize in the face of wealth constraints.

Despite its grandeur of vision and its brilliance of argument and insight, *A General Theory of Exploitation and Class* has some serious failings. Most of these have been discussed fairly thoroughly by a range of critical admirers in an issue of *Politics & Society* (1982, *11,* 3) devoted to Roemer's ideas. Adam Przeworski (pp. 298 and 313) and Erik Olin Wright (pp. 321-341) find inadequate Roemer's account of politics and especially of class struggle. Indeed, Przeworski goes so far as to argue that Roemer makes socialism an unlikely, even unattractive, alternative to capitalism despite his desire to make just the opposite case. Andrew Levine (pp. 343-362) picks at his treatment of justice although arguing that Roemer makes a major contribution to the development of a Marxian theory of justice. Jon Elster (pp. 363-373) claims that Roemer's two definitions of exploitation are "radically different" (p. 362) and defends the first, exploitation as unequal exchange, against the second game-theoretic model. Finally, Douglass C. North and I (pp. 315-320) argue that Roemer's most critical shortcoming is his cursory and off-hand treatment of the state. Although Roemer claims that he has a property rights theory and argues that coercion at the point of maintaining property relations underlies his model, he lacks an actual theory of the state. Thus, ultimately, he fails to account for most exploitation.

All of these critics took Roemer's theory as a building block. There is no doubt Roemer is wrong in many ways, but his mistakes are of the kind from which we learn. At the same time, he has offered as compelling an account of exploitation as yet exists in print. Roemer enables us to understand more about society than we did before, and that is no small contribution.

MARGARET LEVI

University of Washington

Methods in Futures Studies: Problems and Applications. By Brita Schwarz, Uno Svedin, and Bjorn Wittrock. (Boulder, Colo.: Westview Press, 1982. Pp. xi + 175. $16.00.)

Futures studies have found an increasing prominence in the scientific community. Some studies point to the inevitable doom of mankind, while others forecast an unparalleled growth in human potentialities. Popular interest in futures studies has been aroused by the recent deaths of two distinguished futurists, R. Buckminster Fuller and Herman Kahn. Despite the prominence of and popular interest in future studies, potential futures researchers must contend with a dearth of published material dealing with the problems and applications of futures research within a public policy perspective. Although there exists an abundance of policy analysis and social science methodology textbooks, potential futures researchers cannot rely on such texts alone. First, the goal of futures studies is unique: clarifying the range of possible futures and creating images of attainable and desirable futures. Second, the methodological instruments of futures research are many and varied: for example, survey research, forecasting, scenario writing, and mathematical modeling. The authors of *Methods in Futures Studies* fill a vacuum in futures research by providing students of futures studies with a text that addresses the distinctive methodological issues and approaches of this field.

In an early chapter, the authors first address how futures research is distinctive from other scientific research. They next discuss particular methodological approaches characteristic of futures studies such as the Delphi technique, trend extrapolation, scenario writing, and mathematical modeling. Perhaps the most important chapter, though, discusses the problems and practical applications of futures research through the presentation of three cases of futures research through the presentation of three cases of future studies. The first two cases, the Energy and Society and the Resources and Raw Material projects, were commissioned in 1974 by the Swedish Secretariat for Futures Studies. The third case, the Transportation Study, was commissioned by an agency of the Swedish Ministry of Transport. In all three cases, the authors discuss the policy environment, problem formulation, research design, and analytical methods of each project. Especially enlightening is a thorough discussion of the interactive dynamics of the research team in confronting the questions and pitfalls of futures research. In the final chapters, the authors reflect upon their experiences in futures research and comment upon a wide range of topics including the nature of prediction, the role of futures studies and decision making, the importance of the organizational setting, the problems of synthesizing knowledge from different fields, and the value of futures studies.

Overall, this book makes an important contribution to futures research by providing a well-written description of the practical applications and methods of futures studies. It would be an appropriate text for beginning or advanced students of future studies and policy analysis.

MARK R. DANIELS

University of Connecticut

The Civic Tongue: Political Consequences of Language Choices. By Brian Weinstein. (New York: Longman, 1983. Pp. ix + 213. $22.50.)

This book deals most definitely with language rather than speech. However language is not understood in Saussurian terms as a structure beyond human choice and control. In fact, Weinstein's thesis is that the fruitful study of language in political life involves exposing and exploring the many choices that are made about languages, and the consequences that these choices have elsewhere in the social, economic, and political systems. The distribution of languages and their variations can have important consequences by affecting the distribution of a whole range of other burdens and benefits of political life. The primary consequences are in the areas of participation, education, conflict, nation-building, and world politics. Each of these consequences of the distribution of language resources is dealt with in a separate chapter.

The second chapter reviews and rejects the Whorfian thesis that language can shape perception and cognition. Interesting but somewhat unsystematic parallels are drawn between "linguistic determinism" and the political views that underlie some attempts to affect the distribution of language resources in society. The third chapter constructs an ideal model of the language-planning process and reviews the case of French-speaking Quebec to illustrate the language planning process. The fourth chapter deals with the ways in which language may become a political symbol and the various strategies that have been pursued by language strategists in the political process.

The core of the book is Part 3, in which the consequences of language choices are treated. In chapter 5, on participation, cases are detailed that show how language can operate as both an instrument for depriving other groups access to values, both tangible and symbolic, and as an instrument for gaining access to those same values. Next, the dilemmas of education in multilingual communities—particularly in the former colonial areas—are explored. It is concluded that while bilingual education, bridging a minority language and an official language, may have many drawbacks, no other policy seems to be able to reconcile the conflicting interests involved. The next chapter presents and illustrates a taxonomy of the sources of conflict in which language can play a central role. It provides the richest insight of any of the chapters into the interaction between language issues and the social, economic, and other factors that condition political life.

The chapter on nation-building presents an interesting look at the factors that have made the use of language in nation-building an important issue in American political life. The sediment of this unresolved issue is an ambivalence toward bilingual education and the maintenance of distinct ethnic communities. This section is concluded with a discussion of world politics. Recognition is given here that language choices can reinforce cultural, economic, and political relations between states, as well as disrupt or alter them. Aside from the ad hoc theory of the Esperantists that a non-national language will promote peace and cooperation among nations, there has been little successful theorizing that would provide a basis for predicting the consequences of various policies in international communication on the patterns of interaction among nations and the institutions that make it possible.

In his conclusion, Weinstein rounds out the edges of his survey of the literature relevant to the construction of a study of the politics of language. He intimates several of the areas that should be further developed if the study of the politics of languages is to generate the types of problems which make it a vital area of study. He asserts that the study of the politics of language sheds light particularly on elite behavior, but that the masses have the last word. Too little attention is given, however, to the masses' "last word"—and to the social, political and economic factors that condition language choices and mediate the consequences for political life. Finally, Weinstein sets out a rather interesting hypothesis. In speculating on why it is relatively rare to conceive language as subject to political choice, he suggests that perhaps politicians have an advantage in understanding that the choice of one variety of language rather than another for official or other community purposes is an important political weapon, because politicians have a greater sensitivity to the manipulation of individual words to win and hold power. This is an interesting thesis, pointing to a fertile area in the investigation of the intersection of the politics of language and the language of politics—or a core problem of political linguistics.

The strengths of this book are first, that it deals systematically with a large segment of the subarea of political studies we can refer to as political linguistics. And second, this relatively systematic review of the literature related to a topic of concern shows a crystalizing of attention to the complex of problems surrounding the politics of language. One noteworthy weakness is the failure to forge links with other approaches to the study of language and politics, particularly those which can be collected under the label the language of politics. One such possible link—the Whorfian thesis that perception and cognition are shaped in important ways by language—is rather summarily dismissed. Beyond that, much work remains to be done to develop interesting and non-obvious theses about the politics of language and relate this work to the study of the language of politics, and this book provides an important background for that task.

ROBERT PARKS

Elmira College

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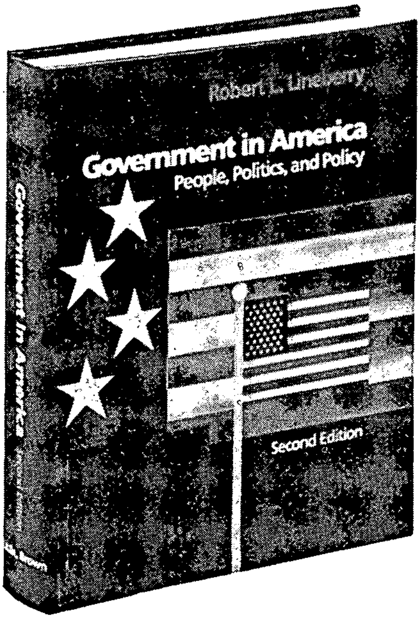

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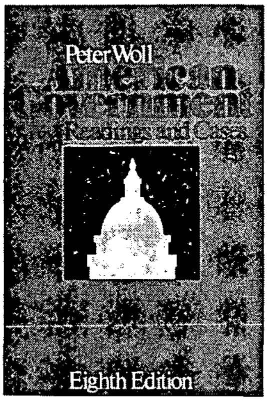

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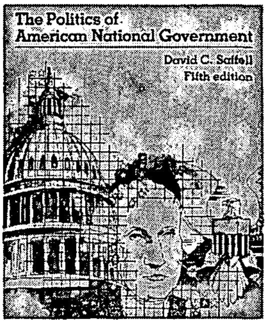

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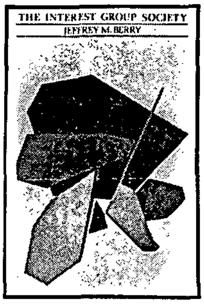

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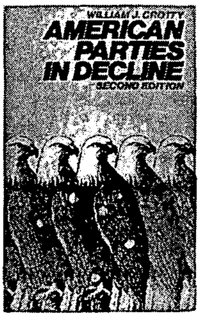

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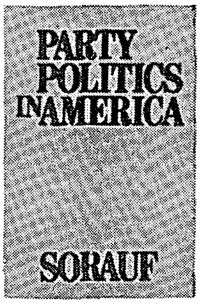

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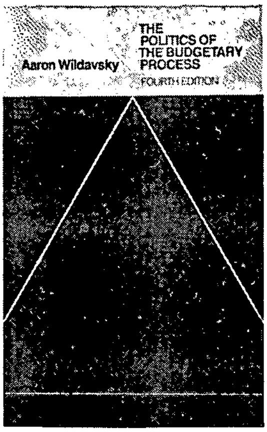
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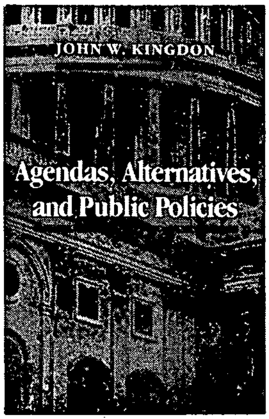
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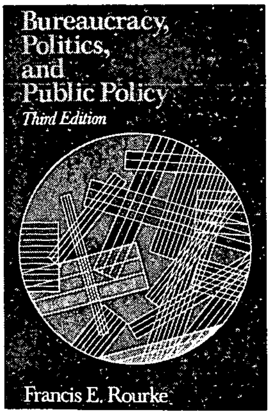
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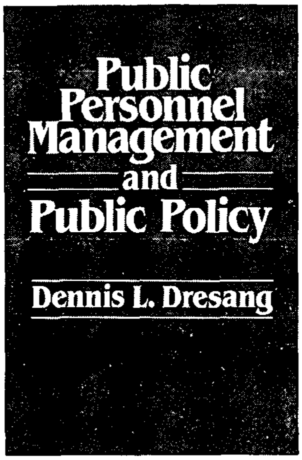
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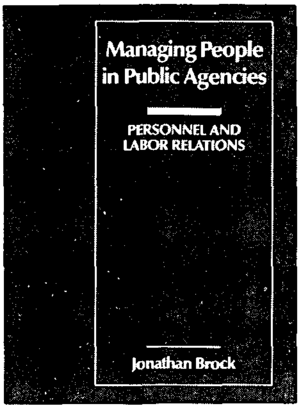
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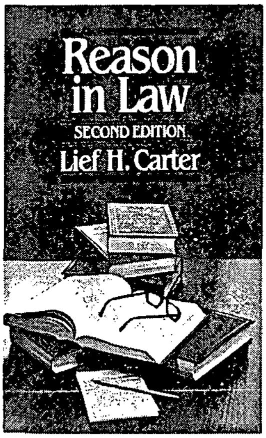
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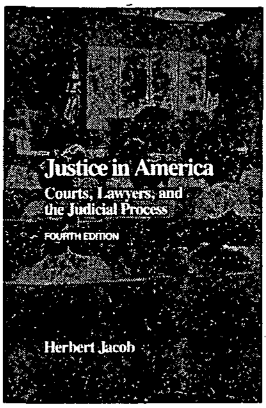
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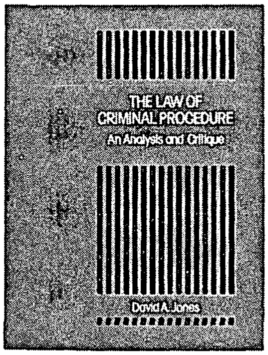
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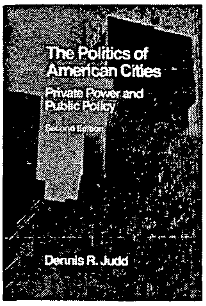
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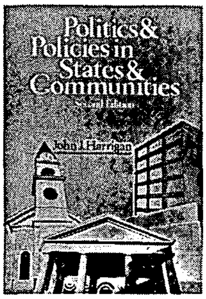
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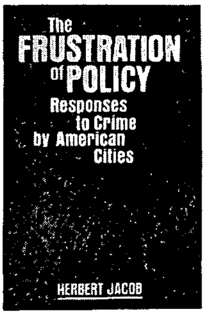
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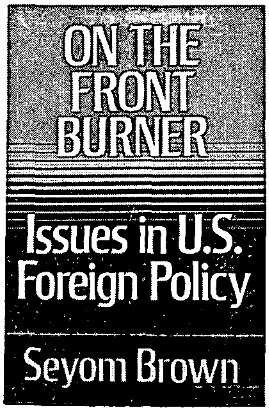

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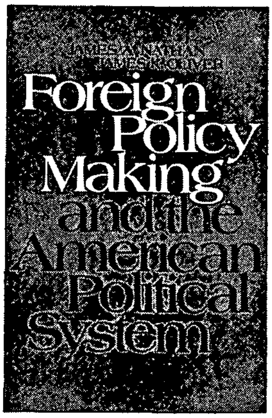

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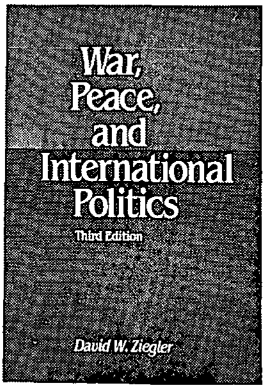

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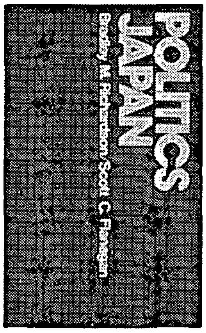
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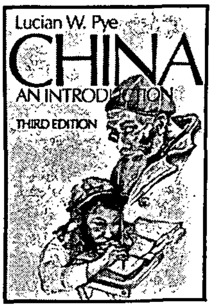
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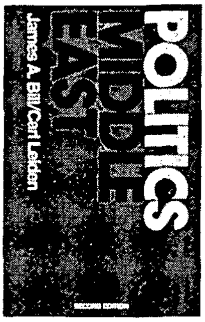
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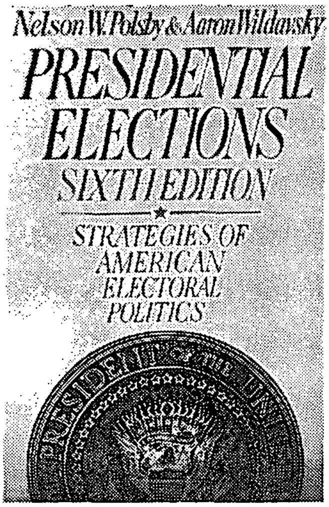
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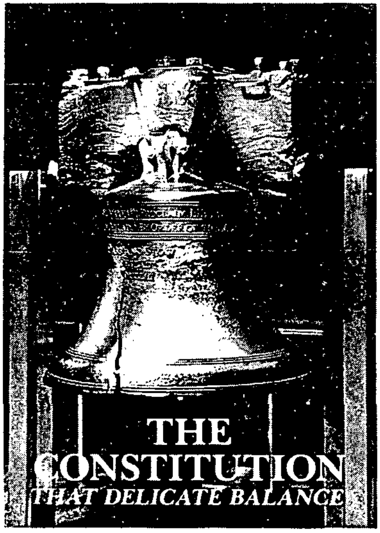

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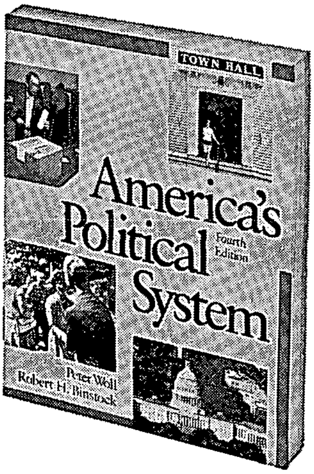

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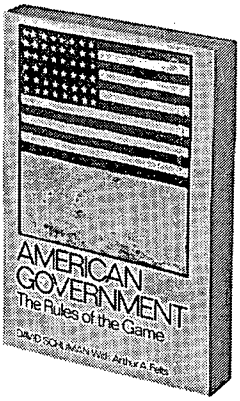

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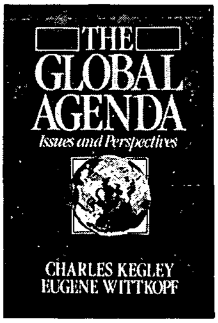
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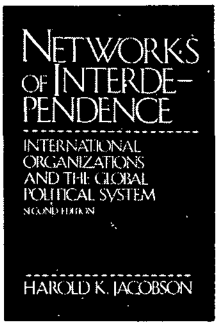
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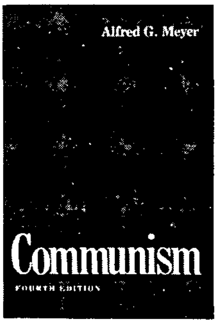
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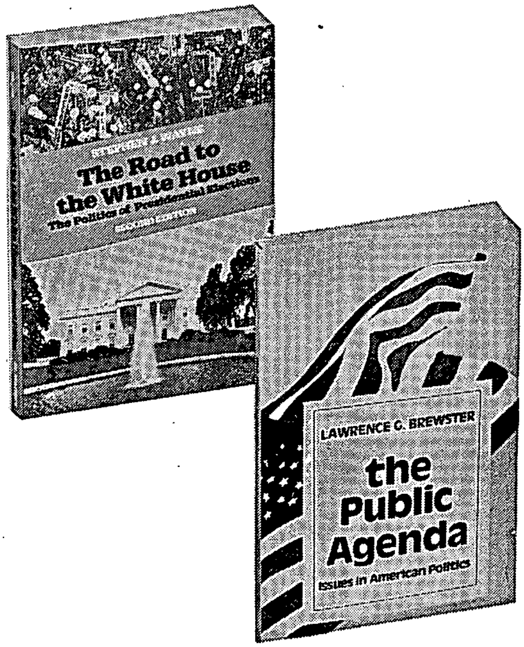
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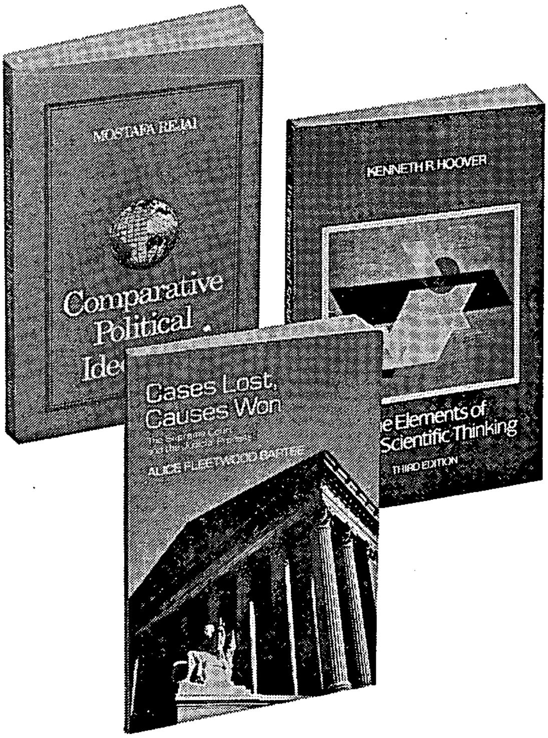

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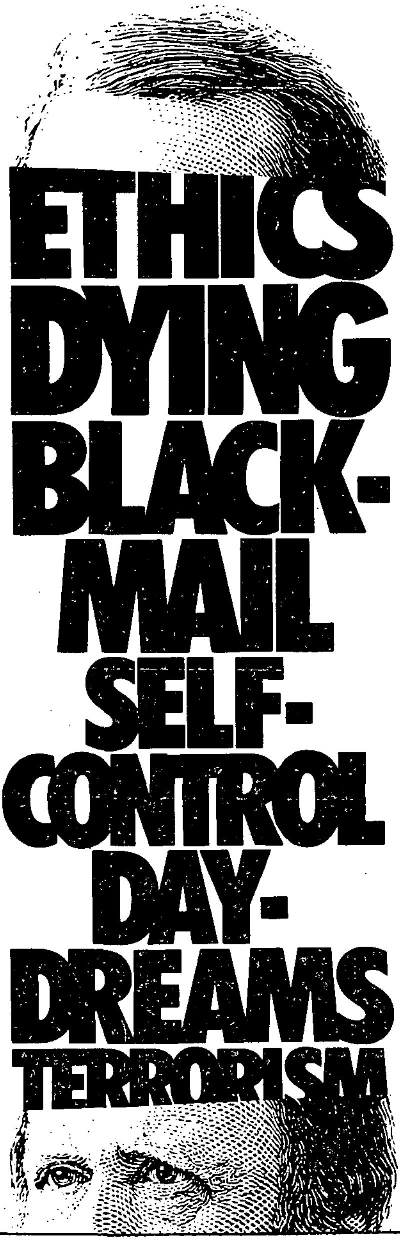

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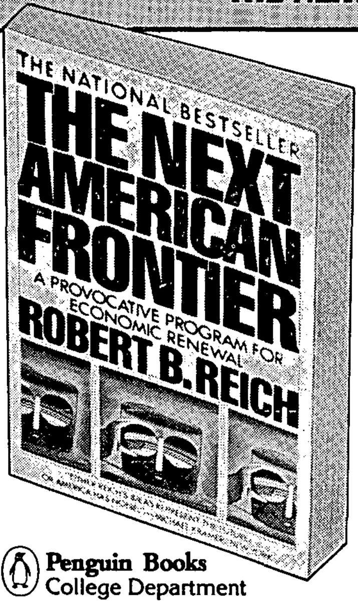
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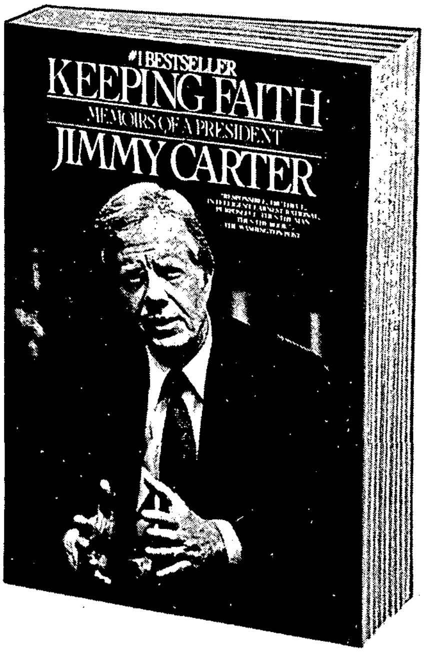

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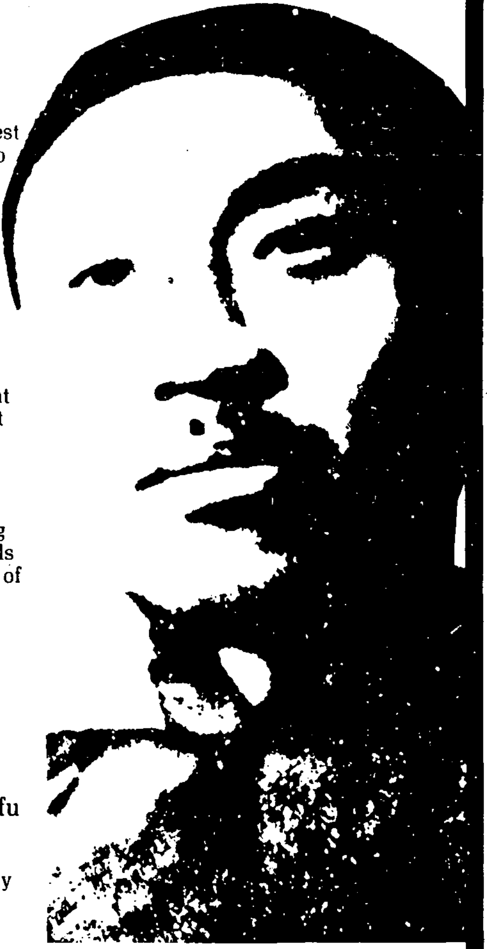

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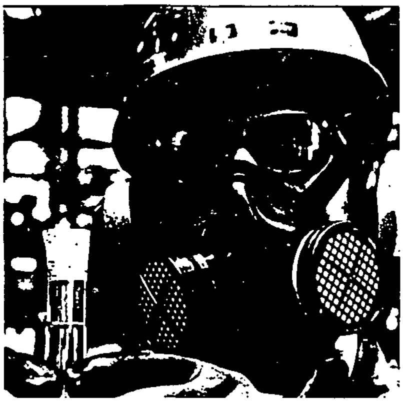

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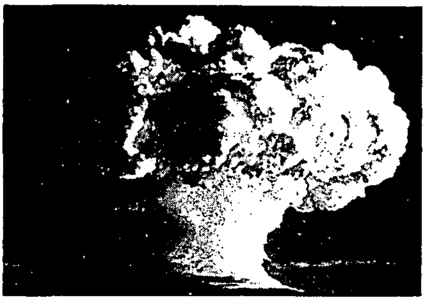

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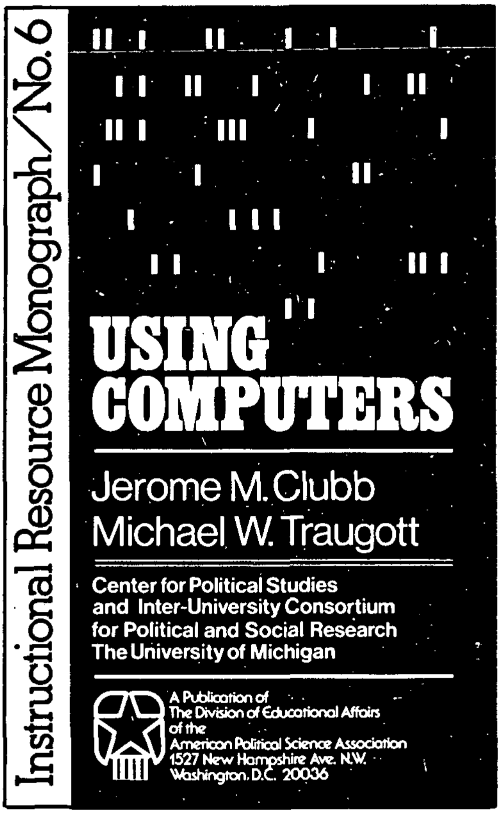

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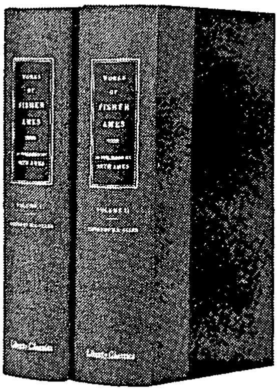

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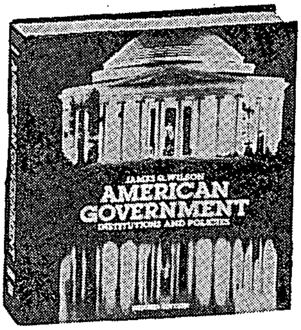

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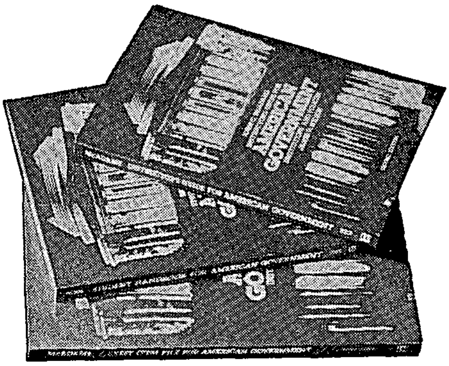

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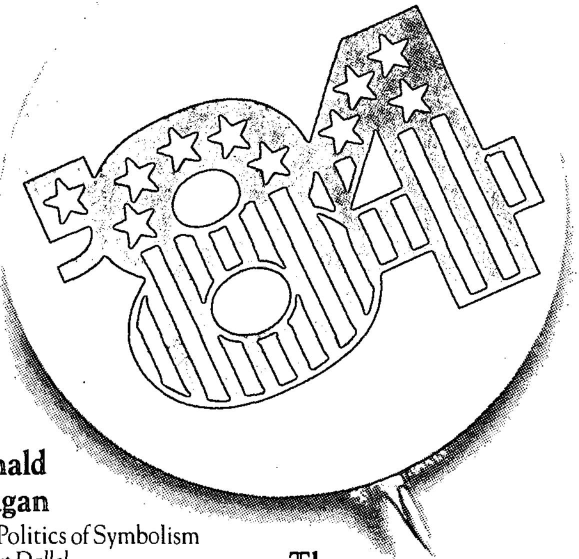

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